Notes on discussion with Tim, April, 1991

1) I argued that founders of QM must have presupposed lti for particles because they used labels, and that is the only way the labels can be understood in QM. Tim points out: In classical phsyics one can introduce the labels on the strength of trajectories, not needing lti. The founders would have been used to using labels from this background and so would have unreflectively made use of them, it not having yet dawned on them that their base, the trajectories, are not available in QM.

2)How present the idea of lti as not parasidic on any real properties?

a) Without labels: Suppose we have 2 things, which agree in all their PROPERTIES. But how then are there 2? They must still differ.

b) Relevance of labels: For it to be intelligible to think of two things as labeled there must be some assymetry between the two. If the two things do not differ in some qualitative property, then it's the residue which counts as the thisness.

c) Get lti by stripping or abstraction. Pare awy all qualitative properties. What is left is lti and can be "combined" freely with propertlies like those stripped.

[By qualitative properties M means properties and relations distinct from properties like =a.]

3) Tim maintains that rejecting LII does not commit one to lti. Think about Black's two spheres which agree in all qualitative properties. In particular there are two trajectories. "I can maintain that B's two sphere's are two but deny that exchange of them through all time would result in a distinct situation."

4) Tim complained that while ltphs has surplus structure, fock space has an unmotivated choice of commutation relations. He clarifies now by saying that the two are on a par with respect to the problem of motivating the theoretical structure. I should acknowledge this — when one erects a structure, parts may have only the weakest motivation of being part of an empirically adequate structure. The point about SURPLUS structure is an additional methodlogical problem, and should be clearly distincuished from the issue of motivation of such structure as there is.

5) Tim claims that the Black's spheres can agree in all qualitative (includes relational) properties, but have two trajectories. It then, he says, makes sense to think of them as switched in trajectories, but without thereby talking about a different possible case — it's still the

same possible case. Thus LII is, in this model, false, with no LTI (the LTI would make the possible cases different after switching.)

6) Is position a parameter or an operator?

Tim says: When he learned QM, he learn position as an operator, one for each degree of freedome. Thus in a two particle system there are 6 position operators. Since position is indefinite in QM, the degress of freedom get associated with an opertor rather than a determinable. Initially he could not get ahold of the structure of QFTh because he kept looking for the position operator degrees of freedom. Then he realized that in QFTh, the degrees of freedom are the field values. Again, since these are indefinite in value, they get associated with an operator. There is one for each degree of freedom, and these degrees of freedom are parameterized by a spatial variable (or a s-t variable). Hence the truth of "In QM position is an operator, in QFTh it is a parameter"

In this way Tim seems QFTh as a generalization of classical field theory in exactly the way that QM is a generalization of classical mechanics. In both cases, identify the degrees of freedom, and then associate an operator with each (and a second with the conjugate variable)

[Note that here I have a way to extend chapter 4's treatment of operators and their comparison with determinables. Determinables are appropriate for quantityes which have exact values and are freely combinable. Faiure of these two conditions calls for replacing determinables with operators (or the physical correlate theroff). I need to investigate the way in which swapping determinables for operators is the appropriate response to vagueness + failure of free combinability, and the connection between these two]

I had a related (though distinct) difficulty. I saw that something got associated with the positions in QFTh. But since it is supposed to be a field theory I jumpted to the conclusion that it is values thus associated, a mistake encouraged by the locution "operator valued field" This is to get the analogy with classical field theory wrong. The point is that the analogy is not exact. One must get the DISanalogy right, and the right disanalogy is exactly the the same as the disanalogy between classical mechanics and QM.

7) Reference on discussions of heicceities:

Armstrong, Combinatorial theory of possibility. A strong and week notion of hecceity, only of of which bear logically on PII. - See chapter 4

Lewis, in Plurality of worlds - a section against heccities.

8) In arguing the relation of hecities and superposition: in individuating two particles, I say that the number-labels create puzzles about superposition. Tim comments that the labels can't be simply dropped cold -- in say, treatment of the singlet state in fock space, one will have to replace the number lables with individuating properties, like right and left.

9) More on heccities: Hecceities do many jobs which need to be separated and sorted out. Tim comments that his notion of heccities is one concerning that which can be freely combined with properties. He takes this to be a stonger notion than mine, which has to do with labeling. In addition Tim identifies three different ways in which one can think of heceities:

1) A principle having to do with reassigning properties to "it"
2) A principle having to do with reidentification in possible worlds.
3) A principle having to do with counting.

Tim also comments that meaauring (in my sense) won't support diachronic individuation. When we have TWO trajectories, with reidentification of parts of the trajectories, we have something stronger than "two" without such reidentification.

10) Tim found my "two kinds of space" unclear. He recommends instead talking about what it is for a particle to have a position. This may be more complicated than trivial assignment of a value of the s-t parameter.

11) Is the peverse reading really pervers? Tim comments that for tangent vectors in a tangent space there is only comparison given a connection, and then only along a curve. So we really do assign different quantities to each of the s-t points. More generally, whenever there is a gauge freedom, since the gauage can be set locally, we again have a different quantity at each point. Remember that we can set the gauge locally and compensate with the gauge field. He agrees that for scalar fields the "peverse" reading is peverse. But what all this shows is that this issue already comes up in classical theory -- it's not new with the issue of the quantum field. This at least needs , first of all, to be acknowledged. Then, it compromises the arguemnt as stated. Since we already know from the classical case that some fields have this perhaps unexpected aspect, it can't be straightforwardly a reason for not taking operators assigned to s-t points to be values of a quantity.

12) Feynman talks about particles taking all paths from start to finish. Somewhere, Feynman comments that thinking this way resolved for him the question of how the particles knows "which way to go" Tim takes this as evidence that with at least one hat on, Feynman thinks of this

realistically . He can't remember the reference, but thought it wan's either QED or Laws

13) Parts: I see two formal difference between mereological parts and analytic parts (vector components) To get an analogy going one needs to analogize disjoint parts with components along orthogonal axes. Otherwise one gets into trouble with saying things like going both north and south are parts of standing still. But then, for a vector in a 2-d space, there can only be two non overlapping parts. and with merological parts one can get into parts of parts, but this goes wrong for vectors.

A second difference is that a mereological part has its own s-t trajectory, not shared with any non-overlapping part. Not so for analytic parts. Tim comments: But quantum systems don't have exact trajectories. I comment: But they do have s-t tubes of "fuzzy position"

King's College London
UNIVERSITY OF LONDON

CHELSEA CAMPUS
MANRESA ROAD, LONDON SW3 6LX TELEPHONE: 01-351 2488

DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

16 July 1990

Professor Paul Teller,
Department of Philosophy
(M/C 267),
College of Liberal Arts
and Sciences,
1524 University Hall,
Box 4348,
Chicago, Illinois 60680,
U.S.A.

Dear Paul,

Schrödinger referred to the money-analogy orally in Alpbach, but he also brings it into the last chapter of

Science, Theory & Man: 'What is an Elementary Particle?'

The legal term in 'fungible'.

Yours,

Heinz.

Dec 30 1990

Dear Paul: Thanks for your letter. About the handling of the negative energy states, whether approach you and/or you are going to have to make some assumptions - this is physics, after all, not logic, and you can do anything from definitions. So the interpretation of K as the e-m 4 vector is just that - doing physics (i.e. making a physical assumption.) I certainly don't think my way of thinking of things can avoid such assumptions any more than any other way. All I think is that it makes the role of relativity clear in the appearance of antiparticles - and in a uniform way. No matter what field - even the Dirac field, the same 4 dimensional fourier decomposition takes place.

Anyway, I just wanted to wish you a merry Xmas and a happy new year - even if it's a little late. And, of course, I wanted to send you a picture of my two guys. I can't believe how big they are getting. Daniel is fully mobile now and although he doesn't talk yet, he makes all of needs and can certainly communicate. His interests in anything - especially if it is in Jenny's hands! Jenny, on the other hand, has stopped talking since he began and has started doing logic. In a recent paper "Jenny Thinks" he declared "The thunder went to another country" from the two phrases "the cat/dog went away to another country" and "the thunder went away". He has also conjectured that the mouse living in the basement likes to run out at night.

Yi finally became a permanent resident of the U.S. so we don't have to worry about that and she is now a mathematics graduate student at Rutgers, and deep into Algebraic topology or something like that. Really, I'm envious - she has gone way beyond me.

Take care

Dale.

What is beautiful
is a joy for all seasons....
OSCAR WILDE

Wishing you the special joys of Christmas.

Dear Tim,

This issue involves enough notation I'm going to write rather than talk about it

We agreed that LIT is irrelevant to superposition of the properties of one particle, and that it does provide additional puzzlement for superpositions of particle number. I then urged that LIT also gets in the way of understanding another kind of superposition - exemplified by the singlet state. For $|3_+\rangle_1 |3_-\rangle_2 + |3_-\rangle_1 |3_+\rangle_2$ [$|3_i\rangle_j$: j is the particle label] the labels - and the presumption of LTI which goes with them - make us wonder: if there is a particle #1 with its identity distinct from that of #2, ~~how can we have a kind of indefiniteness in, and~~ how can we have an indefiniteness in which is 3_+ and which is 3_-? This is different from one particle superposition. Particle identity is not involved in wondering what is involved in one particle being indefinitely at slit A and/or at B. But here we have exactly one 3_+, exactly one 3_- but indefiniteness in which is which. If there are two, identity bearing individuals, #1 and #2, then one ought to be the 3_+ one and the other the 3_- one. Conversely, give up the identity and the puzzle disappears.

To which you countered: Let position - right (R) and left (L) - do the individuating. Then we are back to the case of ordinary superposition of properties for which we agree LTI is irrelevant.

But that doesn't work, I said on the phone - think of R and 3_+ as one property and then

we will see that symmetrization (I'll just consider the symmetrization case) will still require exactly the same thing as $|3_+\rangle_1 |3_-\rangle_2 + |3_-\rangle_1 |3_+\rangle_2$. But I wouldn't see it all clearly before writing it out.

Start with one particle which can have position (R or L) and spin (3_+ or 3_-). This particle's states are represented in the tensor product $H_\Phi \otimes H_S$ [H_Φ - position space, H_S - spin space]. A typical basis vector is $|L\rangle|3_+\rangle$ and a typical superposition is $|L\rangle|3_+\rangle + |R\rangle|3_-\rangle$ - superposition of the ONE particle up left with clockwise-rep. So far there is only one particle, & nothing to coordinate with R & L.

To use the Hilbert space formalism to represent two particles we need copies of H_Φ and H_S labeled with the particle labels: The H-space now is $H_\Phi^1 \otimes H_S^1 \otimes H_\Phi^2 \otimes H_S^2$. To use position to coordinate the particles in a superposition we would need to use vectors such as $|L\rangle_1|3_+\rangle_1 |R\rangle_2 |3_-\rangle_2 + |L\rangle_1 |3_-\rangle_1 |R\rangle_2 |3_+\rangle$ - that is we let position and particle # co-vary. But this is just what the requirement of symmetrization won't let you do! The above is not symmetric under label permutation. ~~The required symmetric state is~~ ~~$|L_1\rangle_1|3_+\rangle_1|R_2\rangle_2|3_-\rangle_2 + |R\rangle_1|3_+\rangle_1|L\rangle_2|3_-\rangle_2$~~ : $|L\rangle_1|3_+\rangle_1|R\rangle_2|3_-\rangle_2 + |L\rangle_1|3_-\rangle_1|R\rangle_2|3_+\rangle_2$ + other with 1,2 exchanged. THIS involves the "one particle property" ~~P, applied to two particles~~ ~~that is $P' = P(1)P(2) +$~~ P : $|L\rangle|3_+\rangle + |R\rangle|3_-\rangle$, applied to two particles : $P(1)+$

To get the required symmetric state, and see the connection with the interpretive problems, factor (1):

$$|L\rangle_1 |R_2\rangle \left(|3+\rangle_1 |3-\rangle_2 + |3-\rangle_1 |3+\rangle_2\right)$$

Note that the term in parens. is just the expression which I argued raises the new interpretive problem. The $|L\rangle_1 |R\rangle_2$ term is your way of resolving the problem by individuating 1 and 2 by positions R and L. But now symmetrization requires replacing $|L_1\rangle |R_2\rangle$ by $|L\rangle_1 |R_2\rangle + |R\rangle_1 |L_2\rangle$, which simply provides another instance of my problem all over again.

Of course we get none of this mess by expressing everything in Fock space. In a F-S representation position does do just the individuating you want it to do, in the LTI free way you want it to. If $\hat{a}^*_{L+}$ is a raising operator for a spin $\frac{1}{2}$-up on the left, etc. The state is $(\hat{a}^*_{L+}\,\hat{a}^*_{R-} + \hat{a}^*_{L-}\,\hat{a}^*_{R+})|0\rangle$. But that's just my point: We get a new problem of superposition brought on by the labels and made particularly acute by the LTI suggested by the labels, which we avoid entirely by denying the labels, and with them, any suggestion of LIT.

Hope I can boil this down to a 4-sentence footnote for the book!

Let me know what you think

Cheers!

Paul.

Comments on Paul Teller's
An Interpretive Introduction to Quantum Field Theory

Comment on these remarks: what follows are the thoughts and reactions I had on several readings of the manuscript. I cannot divide these into changes which I think must be made and those which are optional: I leave it up to the judgement of the author to gauge the seriousness of the problems. Some of the objections strike rather deeply at the philosophical interpretation offered, and would best be met, in my view, by fairly extensive reworking of the material. Short of that, a hope that some acknowledgement of alternative approaches can be made. The length of these comments should be taken as a mark of the challenging and provocative nature of the book. There are an amazing number of typos in this manuscript- to save space I will write the page number, the line number, followed by "b" if counted from the bottom of the page, and the word as it should be.

Pre-Chapter 1

✓ The overall structure of the book is rendered a bit awkward by the use of a free-standing philosophical paper as the first chapter. Having been prepared for an introduction to quantum field theory, the reader is shocked to find the book beginning with a discussion of the use of labels in quantum mechanics. I think the addition of a Chapter 0, explaining that the examination of an interpretative philosophical problem can motivate a choice of the formalism for QFT, would be helpful.

Chapter 1

Before going into the details of chapter 1, let me register my basic complaint. The point of chapter 1 is to examine the consequences of LTI as it seems to be implicated in the use of tensor product Hilbert spaces. That is fine. But the attempt is also made to suggest that the LTI inherent in this formalism is inherited, in some obscure way, from a presupposition that classical particles have LTI (and hence that quantum "particles" must have). This is false. It is no part of the classical conception of a particle, either explicitly or implicitly, that particles have LTI. No results of classical mechanics, no element of its formalism, requires this. A classical physicist who recognizes the existence of intrinsically identical particles need not believe that a description that results from the exchange of particle labels represents a metaphysically distinct possibility.

Above see comment to p 64 on configuration space - relevant to Ch 1

The description of classical particles on p. 10 ff. is correct: classical particles are individuated by their space-time trajectories. The combination of continuity of motion and the impenetrability of the particles provide classical particles with *Diachronic Individuality* , that is, there is a fact of the matter about which particle at a later time is identical with which earlier particle. Further, the localization of classical particles ensures *Synchronic Individuality* : one can discriminate particles at a given time. Note that neither of these features is *Transcendental* in Teller's sense. These two features are sufficient to explain the applicability of Boltzmann statistics in classical mechanics. In the initial state of the system the particles can be labeled in virtue of synchronic individuality. After equilibrium has been reached, states that differ by permutation of particle labels are different, for they can only be achieved if different trajectories had occurred.

It is notable that diachronic individuality is sometimes explicitly denied of quantum particles. For example, Baym, *Lectures on Quantum Mechanics* p. 400 ff. attributes terms in the scattering cross-section of identical particles to the fact that one cannot distinguish which of the outgoing particles is identical to each of the ingoing. Taking this a step further (in a way sympathetic to Teller) we can say that there is no fact of the matter about which one going in is which coming out: quanta don't always have diachronic

To get the required symmetric state, and see the connection with the interpretive problems, factor (1):

$$|L\rangle_1 |R_2\rangle \left(|3+\rangle_1 |3-\rangle_2 + |3-\rangle_1 |3+\rangle_2\right)$$

Note that the term in parens is just the expression which I argued raises the new interpretive problem. The $|L\rangle_1 |R\rangle_2$ term is your way of resolving the problem by underdetermining 1 and 2 by positions R and L. But now symmetrization requires replacing $|L_1\rangle |R_2\rangle$ by $|L\rangle_1 |R_2\rangle + |R\rangle_1 |L_2\rangle$, which simply provides another instance of my problem all over again.

Of course we get none of this mess by expressing everything in Fock space. In a F-S representation position does do just the underdetermining you want it to do, in the LTI free way you want it to. If $\hat{a}^*_{L+}$ is a raising operator for a spin 3-up on the left, etc. The state is $(\hat{a}^*_{L+}\,\hat{a}^*_{R-} + \hat{a}^*_{L-}\,\hat{a}^*_{R+})|0\rangle$. But that's just my point: We get a new problem of superposition brought on by the labels and made particularly acute by the LTI suggested by the labels, which we avoid entirely by denying the labels, and with them, any suggestion of LIT.

Hope I can boil this down to a 4-sentence footnote for the book!

Let me know what you think

Cheers!

Paul.

Comments on Paul Teller's
An Interpretive Introduction to Quantum Field Theory

Comment on these remarks: what follows are the thoughts and reactions I had on several readings of the manuscript. I cannot divide these into changes which I think must be made and those which are optional: I leave it up to the judgement of the author to gauge the seriousness of the problems. Some of the objections strike rather deeply at the philosophical interpretation offered, and would best be met, in my view, by fairly extensive reworking of the material. Short of that, a hope that some acknowledgement of alternative approaches can be made. The length of these comments should be taken as a mark of the challenging and provocative nature of the book. There are an amazing number of typos in this manuscript- to save space I will write the page number, the line number, followed by "b" if counted from the bottom of the page, and the word as it should be.

Pre-Chapter 1

✓ The overall structure of the book is rendered a bit awkward by the use of a free-standing philosophical paper as the first chapter. Having been prepared for an introduction to quantum field theory, the reader is shocked to find the book beginning with a discussion of the use of labels in quantum mechanics. I think the addition of a Chapter 0, explaining that the examination of an interpretative philosophical problem can motivate a choice of the formalism for QFT, would be helpful.

Chapter 1

Before going into the details of chapter 1, let me register my basic complaint. The point of chapter 1 is to examine the consequences of LTI as it seems to be implicated in the use of tensor product Hilbert spaces. That is fine. But the attempt is also made to suggest that the LTI inherent in this formalism is inherited, in some obscure way, from a presupposition that classical particles have LTI (and hence that quantum "particles" must have). This is false. It is no part of the classical conception of a particle, either explicitly or implicitly, that particles have LTI. No results of classical mechanics, no element of its formalism, requires this. A classical physicist who recognizes the existence of intrinsically identical particles need not believe that a description that results from the exchange of particle labels represents a metaphysically distinct possibility.

Above see comment to p 64 on configuration space - relevant to Ch 1

The description of classical particles on p. 10 ff. is correct: classical particles are individuated by their space-time trajectories. The combination of continuity of motion and the impenetrability of the particles provide classical particles with *Diachronic Individuality* , that is, there is a fact of the matter about which particle at a later time is identical with which earlier particle. Further, the localization of classical particles ensures *Synchronic Individuality* : one can discriminate particles at a given time. Note that neither of these features is *Transcendental* in Teller's sense. These two features are sufficient to explain the applicability of Boltzmann statistics in classical mechanics. In the initial state of the system the particles can be labeled in virtue of synchronic individuality. After equilibrium has been reached, states that differ by permutation of particle labels are different, for they can only be achieved if different trajectories had occurred.

It is notable that diachronic individuality is sometimes explicitly denied of quantum particles. For example, Baym, *Lectures on Quantum Mechanics* p. 400 ff. attributes terms in the scattering cross-section of identical particles to the fact that one cannot distinguish which of the outgoing particles is identical to each of the ingoing. Taking this a step further (in a way sympathetic to Teller) we can say that there is no fact of the matter about which one going in is which coming out: quanta don't always have diachronic

individuality. This fits quite well with the analogy of waves offered on p. 33. Waves don't *always* have diachronic individuality: when they interact, questions of identity with the incoming waves is moot. But waves *sometimes* have such individuality (when there is no interaction). Similarly, a single quantum particle, like a electron, seems to be individuatable when not interacting with others (think of an electron in a Penfield trap). It is notable that the question of statistics only arises in states of equilibrium, i.e. in states that are the product of extensive interaction. If identical quantum particles do not maintain individuality through time when interacting, the appropriateness of Fermi or Bose statistics may be vindicated.

It is worthy of some consideration that David Bohm's interpretation of quantum theory is capable of recovering Fermi and Bose statistics even though Bohm postulates classical particles, particles which have definite positions at all times and follow definite trajectories. Thus the classical notion of a particle can be reconciled entirely with all characteristically quantum mechanical phenomena. In the case of statistics, this is done by denying the equiprobability of distinct states, and the equiprobability fail to hod because of the non-local quantum potential. A Bohmian QFT can similarly predict all quantum effects while postulating the existence of a perfectly definite field configuration at all times.

In any case, the attempt to foist LTI on classical metaphysics is both ill supported and unneeded. The important point is that LTI is implied by the tensor product formalism.

p. 9, l. 11 "be". First paragraph: LTI is not *needed* for labeling of particle to make sense as the classical case shows. All I need to do is attach labels at a time and the diachronic continuity will carry them forward. Second paragraph: the denial of the PII does *not* commit one to LTI. The relevant attributes for the PII are taken to be purely qualitative. I may hold that individuals are individuated by particular relations *to other individuals*, and hence by non-qualitative properties. I can maintain that Black's two spheres are two but deny that exchange of them *through all time* would result in a distinct situation. Further, the fact that an entity's individuality is not *fixed* by its qualitative attributes does not imply that it is *independent* of those attributes, as the gloss suggests.
p. 10- Note the "admit of reidentification" of para. 2- this is just diachronic individuality.
p. 11- Again, I deny that classical physics must admit the possibility mentioned in para. 2. We don't need more than "ersatz LTI".
p.12, l.1- "bare"; last para.- again, "particle" does not carry connotations of LTI; l. 2b-"properties represented by".
p. 14, l. 3b- "represented"
p. 15, bottom to top of p. 16: please provide an example of the "different statistical predictions" mentioned. Further, more support must be given to the claim that LTPHSF describes too many states: this really requires the close discussion of surplus structure.
p.16, l.2- "supposed"; l. 3b "exhaustive"; bottom of page- some justification for the equiprobability assumption is needed. This is, in general, a difficult issue, and is just glided over here. Clearly this whole discussion depends on it.
p. 20, l. 3- "exemplified"; l. 6b- "roundabout"
p. 21, l.3 -"compromises"; l. 8 - "restriction"
p. 22- the idea that if two particles have identical *intrinsic* properties then all observable must be symmetric under label exchange is problematic. Suppose I have an electron in a box here and another in a box on the moon. Then the observable according to which the one here has property A and the one on the moon has property B is distinguishable from the one where the particle here has B and the other has A. This is so even though they are intrinsically identical. This of course breaks down if the two particles interact. Perhaps you mean to argue that I cannot know whether the one I put into the box here is the same one that comes out of the box here- but at least *prima facie* this seems to be something I can know in some cases.

p. 24, l. 11- "other words"
p. 27, l. 6b- "contains"
p. 28, l. 8- "symmetric"; bottom of page- the example of the black hole does not seem to be one of surplus structure. If there are no black holes, or if black holes are not physical possibilities, then the GTR is just false (breaks down at some point). For the conditions that lead to the formation of black holes according to the theory are surely physically possible. So if nothing physically possible corresponds to the mathematical singularities, the theory is just wrong.
p. 30, l. 11- "labels"
p. 31, l.7b- "structure"; the mention of Fock space here deserves at least a footnote. The reader who has taken up the book to *learn* QFT will be shocked to find that a previous familiarity with the theory is presupposed. One should be reassured that the formalism will be explained in the next chapter.
p. 34, p. 8- "labels"
p. 35, l. 12- "characteristics"; second para.- the appeal to the commutation relations here deserves some comment. The Hilbert space proponent may say: "My theory is fine. It is part of the theory that one does not have non-symmetric states as representatives of reality." To this your response seems to be that one must justify such exclusions. One wants to see what the formalism says about the ontology, and bare exclusions need interpretation. But if we ask how the Fock space formalism manages to ensure the right symmetry properties, the answer seems to be: through the imposition of commutators or anti-commutators. Similarly, the statistics are derived from this. So it seems appropriate to ask what ontological significance the commutation relations have. Where do they come from? What do they tell us about the nature of the items in the theory? Otherwise, they look like another arbitrary restriction on the theory, one put in by hand to get the right numbers. If so, the advantage of Fock space over the simple exclusion of non-symmetric state in LTPHSF is obscure.

check again Ch 2

p. 37 "we can't make sense of the idea of particles with LTI superimposing to give the needed (anti)symmetric states". I don't see what kind of "sense" is required here, and what LTI has to do with it. Superposition is a characteristic quantum phenomenon that appears even when only one particle is at issue. Think of the two-slit experiment. In some way, this is hard to make sense of, but LTI is no part of the problem. In general, the claim is repeated that superposition is somehow more comprehensible if we give up LTI but the connection is never really made.
p. 38- given the comments above on the importance of commutation relations in this formalism and the lack of any ontological interpretation of them, the admissions on this page are welcome. But the promise that "We will return to this issue on a future occasion" is not fulfilled in this book (I think). Some discussion would be welcome.

Chapter 2

p. 42, l. 4b- "exploiting". General comment on the harmonic oscillator analysis: I don't think that the quantization of the electromagnetic field via a normal mode analysis is given a sufficiently clear or sympathetic presentation. One can give a general recipe for quantizing a classical theory: analyze the classical system as having a certain number of independent degrees of freedom; write down the conjugate momenta corresponding to those degrees of freedom; associate operators with these canonical positions and momenta; impose commutation relations on the operators. From this point of view, the normal mode analysis of the electro-magnetic field is quite natural. What are the degrees of freedom of a classical field? One might be tempted to take the field value at each point as an independent degree of freedom, but this is incorrect if there are continuity constraints on the field. All continuous field configurations get a unique Fourier analysis, and the normal modes *are* independent degrees of freedom. Note that when using box normalization, switching from the field-values-at-a-point to the normal modes

goes from an non-denumerable set of variables to a denumerable set. This indicates how continuity imposes constraints on the field values. Thus the harmonic-oscillator analysis in terms of normal modes is an almost inescapable consequence of regarding the object to be analyzes as a *continuous* field.In any case, it may be more convenient than using the field values at each point as the degrees of freedom of the system. This of course does not suggest reifying the normal modes into vibrations of some entity. I will have further comments on this in reference to chapter 5.
p. 43, top- what exactly is the "privileged role" of the momentum basis? Why wouldn't the conjugate momenta associated with any set of independent degrees of freedom work as well? This should be explained. Also, "privileged" is misspelled.
p. 44, l. 7- "lowering"
p. 26, l. 11 et passim- "one-quantum" rather than "one quantum"?; l. 12 "dispersed"
p. 48, l. 7-"additive"
p. 49. l. 12b, 10b, 7b- "additive"; l. 4b-close parenthesis is needed.
p. 53, l. 7-"Bosons"
p. 57, l. 1- Should some remarks concerning the uniqueness of the vacuum be given?; l.11- the parenthesis here is never closed.
p. 62, l.6- "mathematics"; l. 1b- "particle"
p. 63, l. 11b-"eigenvalue"
p. 64, bottom para.- The claim that the claim that in QM position is an operator while in QFT it is a parameter is misleading is misleading (sorry). That is, there seems to me to be a very good reason to make exactly this distinction, a reason which is not brought out in this discussion. This issue is rather important, and I will likely return to it later.

In QM the position variable is really a degree of freedom of the system. That is, position is always *position of a particle* (in labeled Hilbert space) or *position of some quantum or other* in Fock space. This distinction is easily missed if one focuses on the one-particle QM, but is clear in two-particle QM: there there are six position operators, three for each particle. This is reflected in the familiar fact that the wave-function is a function on configuration space, not on physical space. In the one-particle case, the configuration space of the system is isomorphic to physical space, so the distinction can be easily overlooked. This whole discussion does not seem to make this important distinction.

The importance of configuration space in QM also raises a different means of approach to some of the questions addressed in chapter 1. If a system contains identical particles, especially particles which fail to have diachronic individuality, what exactly is the configuration space of the system? I think the point about labeled Hilbert space might be usefully illuminated in this way. If the configuration space of a system with identical particles is smaller than that of a similar system without identical particles (due to the indistinguishability of some of the states)\then use of the larger configuration space may be the source of surplus structure. Since the wave function lives in configuration space, this is a very natural question to ask. It may make contact with the interpretive issues of chapter 1 in a more direct way than discussing LTI.

In QM one can ask questions like- Where is particle A (suppose A to be the only particle of a given kind)? The fact that this can get different answers reflects the fact that the position of particle A is a degree of freedom of the system. Since particle A may not have a definite position, we associate an operator with this degree of freedom. On the other hand, in QFT on does not ask any experimental questions of the form Where is x? Rather, one asks questions like "What is the value of the field *at* space-time location x? The location is thus *not* a degree of freedom, the field value at a location is. Here the locations *are* in physical space. There are no operators for the location of anything, only operators associated with locations, operators for field values or particle numbers, etc. Thus space-time location is a parameter in field theory, not an operator.

There may be some sense in which saying that in QM position is an operator and not a parameter in misleading, but the point made above about the degrees of freedom of the

system, and about the role of configuration space in QM, are essential. It is, I think, a serious mistake not to clearly lay out the truth that lies behind the supposedly suspect dictum. More anon.
p. 66, l. 14- "presentation"; 5b- "attractive" & "ontology"; 1b "call".

Chapter 3

p. 68 first Para.- Are the disclaimers about technical considerations to be ignored needed?
p. 69- I hope that the sense in which QFT associates an operator with each space-time point is clear from my above discussion. The degrees of freedom of a field involve quantities at a given space-time point which can vary: associated with each of these is an operator. This is of course not to say that the *operators* are "out there" in the world. Rather it is to say that a quantum field has a value at a point in space-time in just the same sense as a quantum particle has a position. In each case, the classical theory always assigns a definite value while the quantum theory associates an operator which gives only expectation values for a given state. l. 5b- "generally"
p. 70, l. 2b "where"
p. 71, l. 2b "independent"
p. 72, l. 10- "detail"; l. 11 "should"; l. 12- "U*|k,0>; footnote l. 2- "operator"
p. 73, l. 6b- "a"
p. 76, l. 4- "relativistic"; l. 10b- "Alternative"
p. 77, l. 3- "description"; l. 4- "involving"; l. 5 "description"; l. 8- "oscillators"; l. 9- "description"; l. 10 "describing" l. 10b- "interpret"; l. 7 ff.- I don't see that the argument for obscurity has been made. I think the gloss given above is pretty clear.
p. 78 top para.- there has been a notion creeping in that there is some problem about *superposition* in the traditional approach to QFT or many-particle QM which is resolved by the acceptance of quanta as non-individuatable, an issue quite different from that of surplus structure. I don't see the connection. Superposition is an archetypal quantum effect, one for which there is no classical analog. The problem of interpreting superposition arises quite independently of any problem about individuating particles: think of the two-slit experiment when only one particle is involved. There is no formal problem with superposing states in the same Hilbert space in LTPHSF. There is a problem in superposing states of different particle numbers, but there is a more obvious problem of evolving into states with different particle numbers. If there is more than a formal problem, i.e. an interpretational question about the meaning of superposition, then I don't see how giving up individuatability helps us solving it. If there is only a formal problem of having states with different particle numbers in the same Hilbert space, again I don't see how the renunciation of individuatability is the key to solving this. l. 4- "freed"; l. 7- "alternative"
second para.- It is pleasing rhetorically to see the elimination of individuatability from the classical conception of particle as giving us a mid-ground between wave and particle. But 1) as argued above, if the property in question is LTI, classical particles don't need it. 2) if the property in question is diachronic individuality, why isn't what is left just a classical wave, which, as you yourself state, does not always have diachronic identity? What is *left* of the classical particle? l. 5b- "approach"
p. 79, l. 6b- "quantizes"; l. 4b- "numerically"; last line- quantum waves are not complex valued functions of space and time, they are complex valued functions on configuration space. As noted above, this confusion is hard to avoid in the one-particle case, but the distinction is essential. Also in last line-"complex".
p. 81, middle para.- I can't follow the reasoning about why the heuristic can't be given a rigorous reinterpretation in QFT. Bottom of page following- see note above on the "myth". l. 11- "equations"; l. 14 "substitution".
p. 83, l. 2b- "Schrodinger"

p. 84, l. 4- "seems"; The tenor of the discussion of positive and negative energy solutions that follows here uses a heuristic: negative energy solutions are not avoidable if the basic field equation (such as the Klein-Gordon equation) is quadratic in time. Although the heuristic is tempting , it is limited and perhaps does not give the best explanation of the origin of the terms. It is easy to see that this must be so since the negative energies also appear from the first-order Dirac equation. Further, if the solutions were mere artifacts (as in the "Annie is half as old as the square of her age 10 years ago" problem) then they could just be discarded as such. It is more natural to begin with a relativistic space-time and an analysis of the field into plane waves with time-like momentum four-vectors. one then gets the usual analysis into positive and negative frequency/energy equations by a change of the integration parameter on the negative frequency part. It is important to approach the question this way so that one can see the necessity of the negative frequency solutions: without them we don't have a complete basis for decomposing the field.
p. 85, l. 7b- "positive"; footnote l. 1- "frequency"; l. 6- "treatment". The interpretation of id/dt as the energy operator needs some justification. The Hamiltonian is the generator of infinitesimal time translations, but is not always the energy operator of the system.
p. 86, l. 11b- "Gordon", "inner"
p. 87, l. 1b- "matrices"
p. 90, l. 10b- "Hamiltonian"
p. 91, l. 2- "satisfying"; l. 5b- You don't want Psi to be an operator, do you?, l. 2b- "reinterpreted"
p. 92, l. 15- "frequency"
p. 93, l. 15b-"particle"; 12b- "frequency"; 6b- "momentum"; 2b- "tantamount"
p. 94, l. 1- "positive"; l. 8- "corresponding"; l. 3b- "Ultimately"
p. 96, l. 3- "presentations"; l. 5- "quantization"; l. 7- "solutions"; l. 6b- "valued". Comments on this last paragraph and following- this is what I was waiting for all along. Two questions. First- why is this only valid for a real-valued field? Second- why use "position" in scare quotes as opposed to, e.g., "degree of freedom"? In general, as is obvious, I don't think that the power of this approach to understanding QFT has been given adequate attention. This gives us a formally exact analogy between the transition from classical mechanics to quantum mechanics and the move from classical field theory to QFT. There may be puzzles about the sense in which a quantized field is really a field, but they are exact analogs to the question of the sense in which a quantized particle is a particle. The comment on bottom of p. 97 about how this line of approach is "at best, analogical only" is obscure to me. Of course there is no question of material oscillators being subject to quantization. But the purely classical Fourier analysis of a field into normal modes is also not a matter of resolving it into material oscillators. The resolution into independent degrees of freedom is exact, and the quantization of these is by the usual means.
p. 98 l. 2- "parasitic"; l. 6- "thoroughgoing". In this para. I don't know what the cash value of "more nearly physical" is.
p. 99, l. 2- "quantized"; l. 8- "absence"
p. 100, l. 7- "noteworthy"; l. 14- "positive"
p. 101, l. 10b-"positive"
p. 103, l. 3b- "superposition". The argument here is a bit obscure to me. If the $|x\rangle$ are not localized in a Lorentz invariant way and the $|x\rangle_{NW}$ are, why not think that, if reality is Lorentz invariant (as it were) then the only true representation of localized states is via the $|x\rangle_{NW}$? I don't see how "two sorts of physical space" get into the analysis- rather there is only *one* sort of physical localization. It will, of course, be differently expressed in different bases. Of course, we might just abandon the notion that

localization is an objective (frame-independent) property of a state- this is not too implausible in any case.
p. 104, l. 7- "reference"

Chapter 4

p. 106- Clearly, the analog of a specific field configuration in classical field theory is not the set of field *operators* of quantum field theory, but rather the *state* upon which the operators operate. Similarly, the analog of the state of a classical particle in quantum mechanics is not the position *operators* but the state upon which the operators operate. In classical mechanics, each state of the system specifies the positions of the particles, in QM only probabilities are determined by the state. Similarly, in classical field theory, the state specifies the exact values of the (spatio-temporally indexed) field variables; in QFT the probabilities for these quantities are returned by allowing the associated field operators to act on the state. Again, QFT stands to classical field theory as QM stands to classical mechanics. The field *operators* are not the analogs of the field, they are the analogs of the classical *functions* that map the state of the field to definite quantities for particular field variables. Hence I deny that the operators represent a physical quantity in QFT- the quantities are represented by the state.
p.107, l. 7- "different"; the second paragraph- the idea that non-self-adjoint operators can represent physical quantities (better- physical degrees of freedom) via the real and imaginary parts separately is obscure to me. How could these be measured?
p. 108, l. 11b- "some"
p. 109 ff. I would replace "determinable" with "degree of freedom". In classical theory, each degree of freedom takes a definite value in all physically possible states; in quantum theory this is not so. The formal representation of a degree of freedom in quantum theory is an operator, the representation of the specific state, the analog of a classical field configuration, is the ket. Thus, again, the main difference is that in quantum states the degrees of freedom are not all assigned definite values. l. 3b- "configurations"
p. 111, second para.- there is a funny shift here from ontology to mathematics. Classical field theory has mathematical objects that represent physical states. There is then a mathematical function from states to real numbers that specifies the value of a particular quantity in a given state. Similarly, QFT has such a function- not to real numbers but to probabilities over measurable sets of real numbers. Thus there are clear *mathematical* analogies. The question of what in reality corresponds to the mathematical entities is quite important, but comparing ontology with mathematics in the two cases is not a fair comparison.
p. 112, l. 11- "ignore"
p. 113, l. 11- "picture"
p. 115- I hope the above presents a reasonably clear alternative to this picture. l. 11b- "temporally"
p. 116- I don't at all see what is "perverse" about this interpretation of the classical field. If I understand correctly, the perversity arises because one regards the field not as the instantiation in various places of one universal property but as the instantiation of physical quantities that are tied to each space-time point. But this is just what classical theory (including GTR) says! Think, for example, of tangent vectors. In the Euclidean context, one may mistakenly think that the same tangent vectors may be instantiated at different points. In the non-Euclidean manifold, however, we see that in general the notion of comparing vectors in the tangent spaces of different points is not well defined. Vector-types are confined, as it were, to a tangent space; we can't see vectors at different points as instances of a single universal. Comparison of vectors is only possible with the addition of a connection, and even then the vectors cannot be compared *per se* but only as transported along a path. Much more about the nature of gauge theories could be said here. l. 5- delete "a"; l. 2b- "breaks"

p. 118, l. 9- "determinable"; l. 11b- "<Phi|"
p. 119, l. 15- "(1),"

Chapter 5

p. 126, l. 7- "after all"
p. 128, bottom- To call the perturbation series expansion "wishful thinking" seems quite unfair. It is, of course, only useful for small perturbations, but the general method is quite respectable in those cases. I feel that some more tangible results are needed to justify the rather dismissive tone here.
p. 129, l. 2- "constructive"; footnote l. 3- "strength"
p. 137, ff. The discussion of "discarding" the vacuum field expectation values suggests that this is just a stop on the way to formulating the correct theory and so the vacuum expectation corresponds to nothing real. However, some physical phenomena, most notably the Casimir effect, appeal to the vacuum expectation. It is not clear that zero vacuum expectation is physically correct.
p. 139, l. 5b- "infinite"
p. 140, l. 5- "Feynman"; l. 6- "mileage"
p. 145, l. 1b- "walking"
p. 146, l. 5- "diagrammatic"; l. 7- "diagrammatic"; l. 11"representing"
p. 147, l. 12b- "description"; I don't really understand the objection to using the terms "creation" and "annihilation". Even if one gives up LTI, the result of the operator acting on a state is to yield a state with one more or less quantum, thus creating of annihilating one. Nor, so far as I can see, does the fact that states can superpose effect this. Thus I find the discussion on the next page unconvincing.
p, 149- One may not want to take the sort of ontology that Feynman associates with his diagrams seriously, but at least the position can be given a more sympathetic rendering. Feynman sometimes speaks as if the diagrams really do represent actually evolving processes, processes that all take place simultaneously and the sum of which yield the probability amplitudes for observations. These underlying processes would involve creation and annihilation of particles, exchange of mesons, etc. The smooth and deterministic evolution of the resultant amplitudes is not because there are no such creation and annihilation events at the underlying level but because the discontinuities are washed out when we sum up all the contributions. l. 4b- "do"
p. 150, l. 12- "amplitude"; l. 14-"constitutes"; l. 2b- "non-relativistic"
p. 151 ff.- This discussion of parts is, to me, unconvincing. It seems to me that the Fourier components of a wave are as legitimate parts of it as parts of a table are parts. This can only be refuting by providing an analysis of parthood, which is obviously lacking. The analogy to burning sugar on p. 152 is out of place: the Fourier components literally sum to the wave while the combustion products do not make up sugar. It may not be sufficient that the putative parts sum to the object for parthood, but it is pretty obviously necessary.

The analogy on p. 153 is also unfair. If I ask for an analysis of a wave into Fourier components the result is uniquely determined by the wave, it I ask for a resolution of a velocity into the sum of a Northerly and Southerly velocity the result is not fixed. In general we may distinguish two sorts of parts of an object: 1) analytical parts- the result of some unique method of regarding the whole as a sum of parts where the decomposition is into orthogonal components. Analysis of vectors into orthogonal components satisfies this, as does Fourier analysis. Analysis into non-orthogonal components does not. Thus, if we wish to analyze a motion into motion North plus something else we should begin by taking the projection of the velocity vector on a North pointing unit vector. Your actual velocity cannot, therefore, have as parts Northerly and Southerly components. 2) dynamical parts, such as are discussed, where the parts are identified via their causes. I don't see any objection to analytical parts as parts- neither

the sugar nor velocity examples tell against them. Nor is it relevant that a wave can be analyzed by resolution into many different sets of waves. I have a top half and a bottom half, a right half and a left half. I can be cut up in different ways, but these all yield parts of me. This is where a general discussion of mereology is most needed.
p. 153, l. 6- "putting"
p. 154, middle para.- "No hidden variables" proofs are irrelevant here- the question is in what sense the processes depicted by the Feynman diagrams are thought of as existing. According to Feynman they *all* take place. Thus Feynman would not refrain from saying that the particle passes through slit A or slit B- indeed he would insist that it does *both*. Its observed behavior is a result of the joint effect of all the happenings. l. 7b- "individual"
p. 155- considerations of how gigantic the superposition of these possible histories in the sum-over-histories picture is would not move Feynman. The more the merrier. Indeed, it is exactly because in some sense *every* possible history is tried that the view is so appealing: no selection principle among histories is needed. The particle doesn't have to know where to go, it goes everywhere possible all at once. Again, you may not like the picture, but at least it should be presented in all its glory. Thus the "as if this were not enough" rhetoric would not faze Feynman. l. 6- "superimposed"; l. 9b- "Feynman".
p. 156, l. 6- "an", "represented; l. 7- "density", l. 11- "corresponding"
p. 157, l. 4b- "catastrophe"
p. 158, l. 4- "Bethe"; l. 7- "non-relativistic"; l. 8- "Bethe"; l. 12- "Feynman"; l. 8b- "psychologists"
p. 159, l. 11- "suggesting"; l. 8b- "lattices"

Chapter 6

The only comment I have on this chapter regards the justification of the requirement of renormalizability given on pp. 247 (journal pagination) and 255. It is said that a scheme is hopeless if it requires an infinite number of empirically determinable constants. This seems not always to be so. For example, it was once speculated that the families of quarks would constitute an infinitely ascending hierarchy. There is no reason to assume the masses of the quarks would be calculated from first principles. But this is no fatal objection to the theory. If each successive family has a smaller effect on observable phenomena, approximations may be calculated to any desired degree of accuracy. No doubt an infinity of quantities needed to absorb the divergent terms would not be so benign, but one should explain exactly why this is so. Certainly one cannot reject the infinity *per se* since the real infinities approach is accepted as a possible interpretation of the physics.

ADDITIONAL COMMENTS ON PAUL TELLER MS.

I write '5,2,3' for 'p. 5, para 2, line 3', '-3' for 'line 3 from bottom', 'Main' for 'main comment'.

Chap 1

Worth the space to do in detail?

8 & 10: disentangle: nameability, which is linguistic; being a coninuant, TI through time; primitive transworld identity, TI through possible worlds. 'LTI', introduced p. 10 explicitly continues entanglement. Unclear what 'PTI' (set aside) really is.

✓ 10, low: relate Ind Properties to the standard idea of individuation as providing interesting weak sufficient conditions of identity, or equivalently interesting strong necessary conditions of non-identity,

?o reword

11,2,-4: 'makes sense' etc. RElate 'no physical difference' to symmetry of classical Hamiltonian; do you claim that classicalphysics' world-picture allowsa non-symmetric Hamiltonian? cf. also 15, line -5.

✓ 13,2, last 5 lines: penultimate sentence's use of joint state is not felicitous. Refer explicitly to linear combinations.

✓ 13,3: all Hilbert spaces of given dimensuion are isomoprhic; so clarify talk of 'the same Hbt space'

✓ 20, top: This suggests we hardly needed p. 19's list i) to iii). Go straight to the second sentence of iii), and delay i) and ii) till need it at p. 21 mid.

✓ 20, last 3 lines: ugly phrasing.

✓ 22, lines 9-10: expand on distinction between permutation of particle labels and of states.

✓ 23,3,3: ?insert '(unit-length)' before vectors, and similarly elsewhere.

I feel some readers need the repetition of this point

23, last 8 lines: could cut, clear from previous para.

✓ 24, last para: explain 'irreducible' and 'Schur's Lemma'. This page not felicitous: 'maximal' line 7 will be read by newcomer as just meaning that all eigenvalues are non-degenerate.

✓ 26,2,4-5: cut reference to Hartle & Taylor p. 2045: what is here cited is not outof the common run for this exposition, and you are not giving refs at every turn.

notes to readers, point #3

This is a very good pt which I completely missed, will take some thought.

✓ 28mid, & 29 mid: in a philosophical book such as this, surely expand, e.g. with refs to literature on theory-choice, underdetermination of theory by data, and e.g. comparison with Friedman's 'Fdns Sptime Thies' distinction between weak and strong unverifiability,.

29, line &: explain. This is 'superselection rules''s first occurrence, despite p. 24 onwards. The reader is right. But it's such a small point, with no bearing on what follows, I'll relegate to a footnote.

I'll check references and respond as appropriate

✓ 31,2,5 et seq: The proposal is puzzling: How keep track of numbers and distribution (lines 6-8) without a concept of which particle has which properties? Since you say this yourself innext para, ? rewrite.

33, line -1: 'Hardly'. Contentious; you implicitly here reject tropes. Cf. e.g. Lewis Plurality of Worlds, p. 64-69, and refs there to Keith Campbell.

✓ 34, line -4: who is'his'? Teller in ref 8? 'his' was supposed to be 'this' !!

✓ 36, line 5f.: Fock space giving more than LTPHSF. Important to author cos of e.g. 29mid, 32 para 2 end, and the 'reverse direction' project of p.43f. But care is needed: cf. comment on p. 43, of Chap 2. In particular, here, acknowledge that LTPHSF makes no attempt to deal with variable particle number: so clearly distinguish (i) uncontentious point that Fock does so deal; (ii) contentious claim that we can build Fock without putting in symmetrization or antisymmetrization by hand.

Chap. 2

✓41, line 2: 'Here' to end of para sounds too defensive: cut. Ditto for lines 9-11 of 43. *Point particularly well taken.*

✓42, line 6: 'corresponding' should be 'proportional'.

43: 'reverse direction' project. 'MAIN' Cf. last comment on Chap. 1. Please add detail on exactly how much we need to postulate to get a linear structure on H, such that p. 49 line -3 holds, with each H_i isomorphic to the symmetric part of the i-fold tensor product of H, with itself.

These details seem needed, because: even if we DO put in symmetrization by hand, the relation between number repsn and Fock is subtle -- displayed in item (1) of enclosed xerox.

These details would 'finish the job' i.e. vindicate 49 line -3, if Teller could somehow derive from his motivating talk of pp. 47-49, an isomorphism to/postulation of: $\otimes_k L^2_k(R, dx)$.

I'll do my best. This will take some study.

✓44, 2, 9f.: 'MAIN', though pedagogic. 43 para 2's abandonment of harm. oscillators has a price. The physics is all in the eqns of motion of the raising and lowering operators, as fixed by the Hamiltonian of the system. The newcomer may baulk at the abstractness of 44f., which looks like an exercise in linear algebra. p. 64 para 1 shows you will probably dig your heels in on this one. But here are some easy suggestions for how to make amends:

(i) move earlier, perhaps into Chap 2 some of the physicsish material of p. 62f, and p. 90f.

(ii) put e^{-ikx} for <k|x>, to give somelife, on p. 71.

(iii), at p. 62, where you write a_k^* as a creation operator for particle of momentum k, please also write down the action of a_k^* and a_k on an arbitrary vector of Fock, cf. item (2) on enclosed xerox.

The reader seems to fell that the presentation is too abstract + so cut off from the physics. I agree that it should be made more concrete. But more is the correlative danger of the chapter becoming cluttered. I will try to find a happy medium.

✓48, low: nothing in your motivation makes k finite, though arbitrary. That is: why not an infinite sequence of n_1, $|n_1, n_2 \ldots n_i \ldots\rangle$?

✓55, line 10: 'natural'; surely provable by thebasic definition of two operators A B and vector v: c(A + B)(v) =df. c(A(v) + B(v)). *But the interpretation still not forced by this formal*

✓58. line -9: 'motivates' could surely be similarly strengthened

✓63, line 2f: <k|x> is a number not a function: rewrite to get a rigour similar to p. 55 para 2.

Chap. 3

71, low & 74-76: MAIN: good and clear presentationof the field idea, but:

No. (i) after all themephasis on the comm relations of the a'a and a*'s, please deduce the comm. relations of Ψ and its conjugate momentum.

✓(ii) more important: the narrative drive, the point, o fthe rest of the Chap. is unclear. 75 low and 78 low are too sketchy; no reason is really given for the ensuing development. Although I followed exposition of rest of Chapter, 79 top to 105 (I think!), I don't see it how it supports the main interpretative idea of the book, as so far spelled out.

This is done in so many texts. It would just clutter my presentation

I will work hard on this

✓73, line -3: $w(k)^2 = k^2 + m^2$. Say more; introduce 'mass-shell' terminology.

✓76, line 9: explain 'non-local'.

✓77-78: this motivating stuff out of place here; perhaps at start of Chapter.

✓80, fn 5: 'Remember': I don't think you have said it before; cf. fn 3 of p.73.

✓81, line 3: explain 'equality, not identity'. Sim'ly, p. 82 low (84 mid): operators are after all fixed by their action on vectors.

Part of (ii) just above

✓83, top: MAIN, pedagogic: I find it odd to get a Schrod eqn for operator-fields, i.e. (4), before one for complex-number-field, i.e. (9). Cf. also p. 84. This suggests a substantial change of order.

The order is right, but I haven't made it clear why its right. This is part of comment (ii) above.

No96, bottom: explain Laggian formalism here (if not before!) for newcomer

✓98, second quantization: strangely no mention of the N-particle Schrod eqn, and of its extra restriction to a fixed total number of particles.

No101, para 2 end: MAIN. I think this is false, if my memory of Fleming's lectures (attended by Teller & me) is right. Cf. enclosed xerox, marked (α). *I worked closely with Fleming on this. I'll double check, but I'm pretty sure I've got it right.*

other places. This would just add clutter.

Similarly 102, para 2 end is dubious; Cf. enclosed xerox, marked (B) I'll check this too,
What is true is 103 bottom, viz. that in the KG position representation, the estates of NW position have spatial width of Compton wavelength.

Chap. 4

107, para 1 end: be more specific; presumably say that the state will be a vector on which the operators act.

✓108-109: A determinable is usually considered to be itself a property, e.g. being massive, not a collectionof them. Then each physical quantity/magnitude is a kind of determinable property; and p. 111, para 2's putative disanalogy begins to look a lot less strong. Especially when we recall that the spectral decomposition of operators makes them look more like classical determinables (pace p. 111, last line).

✓112, para 2 end: ref. back to previous ignoring.

112, para 3 to 113: sen dthis basic review earlier.

✓113 para 3 to 114 para 2 end: This is one of several passages in the book which read very well and convincingly (though 115 last third is a bit repetitive). Except that the presentation doesn't highlight what the state actual is, cf. comment on 107, above. In other words, it loses sight of what is mentioned well on 113-114, the 'time-independent state' (114, end para 1). And: Please unify with presentation of Fock by writing down the action of the operator field Ψ on elemenst of Fock space. This would also make vivid what you say on 119 para 2.

✓117, line -1: explain 'functional approach'.

✓119, para 2 start: re-exhibit relevant eqn(s) from Chap 3 for clarity and a sense of unity.

✓120, line 4-6: I see no contrast to classical situation here. There also, surely, there are freedoms to transform. In this respect there was no contrast intended. The comment is a technical one to plug a formal loop hole in my broader claim. I'll clarify.

Chap. 5

✓128, fn. 2: This admits to a point that could beclearer on text: viz. the derivation on this page is nothing especially to do with H_I. Please rewrite to say that we take over ideas of standard time-dependent perturbation theory.

✓137, line 5-6: say more about why.

✓139, line -2: to help reader, reproduce the line before (27) of Chap. 3.

✓154, top: It doesn't have to be causal history: cf. mereology, and other literature on parts and wholes, e.g structural universals.

✓147-155: This is one of several passages in the book which read well and convincingly. But perhaps add on whether the virtual particles are on mass shell; and on the significance, for the 'reality' of virtual particles, of the impossibility of turning the interaction off.

157f: This three pages on self-interaction too sketchy, and not integrated into corresponding material in Chap. 6. Probably best to send to Chap 6 and integrate there. I'm not sure about this, but I'll expand & seriously consider the suggested reorganization.

Chap. 6 (already published in Phil Sci; use pagination of article)

MAIN: The Chapter reads well. But I am not convinced that §5's three alternatives are really on a par with one another, as Teller suggests. That is, surely a good case can be made that the mask of ignorance approach is: (i) superior to the other two; (ii) what truly expert and/or reflective practitioners believe to bethe best defence of renormalization. Teller himself gives the materials for the case. Thus cf: 248, para 4's statement that on the cut-offs approach, one suspects something is wrong with theory or approximation; 249, para 5's statement that for any known method of regularization, the regularized theory fails to have some 'good' property. My claim that the mask of ignorance is really what is going on i sbacked up by a recent quasi-historical study, viz. by A Rueger in PSA Proceedings 1990 vol. 1. Teller knows this paper.

The disanalogy is very strong. But the reader shows how I can make my point much more sharply,

But I am more inclined than the reader to avoid details which do not advance my conceptual points.

See cover letter.

2407 Elendil Lane
Davis, CA 95616
February 21, 1991

Dear Steve,

I've thought a very small amount more about your "dumb" question. I haven't had the time to chase it down the way I should right now. But I've got a guess about how the answer may go.

As you lay it out in your letter, one can put the issue in terms of how to justify or motivate the form of the momentum operator, $\hat{p}$. So, begin by making no assumption about the form of $\hat{p}$. The arguments from conservation of momentum amounting to the requirement that $\hat{p}$ be the infinitesimal generator of spatial displacements is supposed to give the result that

$$(1) \quad -i\nabla_x \hat{\phi}(x,t) = [\hat{\phi}(x,t), \hat{p}]$$

(This is the thing I've got to chase down. I've seen these arguments, but I've never understood them at all satisfactorily, and I can't begin to remember how they go.) But from (1) it should follow that the effect of $\hat{p}$ is to multiply the coefficient of e^{-ikx} by k. The standard form for $\hat{P}$ has this effect. I haven't thought through whether it's easy also to argue uniqueness, that only the standard from for $\hat{P}$ has this effect.

This point is glossed over in every text I have ever seen. I suspect that working it out carefully would make a nice little paper. But I'm not going to be able to do it now. Hopefully I can think about it some more this summer. In the mean time I'm really grateful for your having nudged me into thinking this much about it.

Cheers,

Paul

Dear Paul,

My dumb question is about pp's 92-96, the complex K-G equation. The way the argument is set up, it seems to me that the a's and c's - the Fourier coefficients that later turn into operators - originally come in ~~in such a~~ carrying k as an index; the k in $c(k,t)$ tells you only that $c(k,t)$ is the coefficient of $e^{ik\cdot x}$. So far, then, one doesn't know whether $\hat{c}(k,t)$ is going to be a~~n~~ lowering operator, or what momentum it will add. When you get $[\hat{c}^*(k,t), \hat{c}(k',t)] = 2w(k)\delta^3(k-k')$, and the Hamiltonian H_k, then I guess it's clear that $\hat{c}(k)$ is the raising operator, and that it adds energy $w(k)$, but k still looks to me like an index - it isn't yet clear that $\hat{c}(k)$ adds momentum $\vec{k}$, or $-\vec{k}$. So the switch from $\hat{c}(k,t)$ to $\hat{d}^*(k,t)$ seems trivial - just a shift in notation. But you don't treat it that way - rather, you take it as clear that $\hat{c}(k)$ will add momentum $\vec{k}$ or $-\vec{k}$ to the field, and you argue for which one it is.

Now I understand what it is to add energy to a (1-dimensional) harmonic oscillator, but not what it is to add a 3-vector momentum. The momentum $\vec{k}$ is added to the field - i.e. to that object whose various states are expressed by the Hilbert space. But I only understand what it is to add $\vec{k}$ momentum to the field when I know that the momentum operator is i) $\int d^3k\, \vec{k}\, \hat{a}^*(k)\hat{a}(k)$, and likewise ii) $\int d^3k\, \vec{k}\, \hat{d}^*(k)\hat{d}(k)$. It's at the point where you say i) (p. 95) and I guess imply ii) that I think the k in $\hat{d}^*(k)$ is no longer just an index - how one sees that $\hat{d}^*(k)$ adds momentum $\vec{k}$, and how the trick has been worked of pairing a $\vec{k}$-raising operator with $e^{-i(k\cdot x - w(k)t}$. My difficulty is that the entire argument seems to rest on ii) $\hat{P} = \int d^3k\, \vec{k}\, \hat{d}^*(k)\hat{d}(k)$, and I can't figure where that's coming from. Consulting the books I found in the library, I know how you get $\hat{P}$ out of writing down the Lagrangian for ψ as a classical field, but a) you seem to be avoiding that approach, and b) anyway, if you go directly to the Lagrangians, you don't need any arguments about analogies to harmonic oscillators, or switching from raising to lowering operators, etc.

So here's my question. Do you have some way of looking at this that either motivates saying ii) (rather than, e.g. $\hat{P} = \int d^3k\, (-\vec{k})\, \hat{d}^*(k)\hat{d}(k)$ without using Lagrangian methods, or that motivates saying that $\hat{d}^*(k)$ - i.e. the coefficient of $e^{-ik\cdot x}$ - adds momentum k rather than e.g. $-k$ &

via some other argument? What are the conventional treatments you have in mind?

Sincerely,
Steve Leeds

P.S. I like the book so far – I'm only nagging you about this one issue because it's so frustrating to find a book that almost makes it all clear, and then to find myself, as always, confused.

publish.

Well, these are just a couple of points glancing through the manuscript. I shall settle down to a careful read soon, as I have said. I thank God you are around and trying to spread these ideas around, and wake philosophers of quantum theory up to the fact that in 50 years no-one has touched the central theory of fundamental physics, up to superstring theory at any rate (I wonder how many centuries before philosphers get round to that! Except for Bob Weingard, that is). It's funny, right through from antiquity philosphy and physics have kept contact, right up to the late 20's, lagging only a decade at most behind the developments of physics. Just think of it; Locke, Whewell, Mach, Poincare, Reichenbach - philosophers or phil/phys right on the mark. But not a thing since. Well, I suppose there was Hanson with his book on the positron (a bad job); and Nancy Cartwright makes some mileage out of lasers. But basically the last 50 years of physics might not have happened for all philosophers care. The point being, it's getting late to catch up. So as I say, all credit to you for getting this book out, and this *sort* of book as well.

By the way, yes I *have* been here this year; an amazing experience. Totally screwed up, of course, but you fall in love with it all the same. Not just Israel - it's the whole Middle East. The landscape, I suppose, its riviting, constantly breathtaking - but there's something else as well, something to do with Islam, the strangeness and mystery of the people. Israel of course being something yet again. I love it and hate it; admire it and condemn it. You can't separate the politics and the constant threat of war, the chauvinism, the religious fundamentalism (Jewish, that is), from Israel as it is, or even from the Jewish character; but I shouldn't presume to tell you about it, I'm sure you've been here. One thing: the arab states are full of fear, full of deprivation; that's what I've seen - not because of the Israeli threat or anything, because of what they are.

Well, did you get offered the job at Davis? Did you take it? I rather think the answer to both is yes... congratulations if so. Funny to think we were in competition for it. I liked it very much, and do give my regards to Michael Wedin and Richard Healey. I would have been much happier there, I think, but the trouble with Harvard - where I've ended up - is that you feel like a coward not to take a job there if offered. At least that's what I felt. And now of course I'm dead nervous about it.

So, any letters in future - please mail to the Department of Philosophy, Emerson Hall, Harvard, Cambridge, Mass. 02138; I'll be there from September. Anytime you are in Boston please look me up.

Best regards,

Simon

האוניברסיטה העברית בירושלים
THE HEBREW UNIVERSITY OF JERUSALEM

Sidney M. Edelstein Center
for the History and Philosophy of
Science, Technology and Medicine

מרכז סידני מ. אדלשטיין
להיסטוריה ולפילוסופיה של
המדע, הטכנולוגיה והרפואה

29/7/90

Dear Paul

I only recently got your manuscript - for which many thanks. I am looking forward to reading it with keen interest. It came a little circuitously, hence the delay, but I shall be starting work on it pretty soon, or at least that is my plan. Looking over it, it seems a very good job. But I can see immediately substantial points of difference with how I would develop the theory - of course, how could it be different? A couple of points which are strictly technical, though: first, the way you reinterpret the negative energy states in the field theory - when you analyze the *real* KG field, I do think you should say that the (q-number) amplitude for what (in the 1-particle theory) were negative energy states must be a creation operator because it must be the adjoint of the amplitude for the positive energy states (which is an annihilation operator) - this all because the sum of the two must be self-adjoint. This reasoning doesn't apply in the complex case, and I realize you want a heuristic that is quite general; I don't agree with your heuristic, I think it conceals something more important going on, but the way my disagreement leads to (or follows from) a simple mathematical argument is right here, and of course it is related to the fact that there can be no antiparticles for the real field.

ch3

A second point is that you say no-one has discussed the relationship of the x-parameter for the Newton-Wigner eigenstates with the coordinates for physical space; I think it is true that no-one has discussed it in a qualitative and heuristic way, but from a technical point of view there is quite a lot on this, for example in Fonda and Ghirrardi's text book, *Symmetry Principles in Quantum Physics*, 1970, Marcel Deker, New York, or in Varadarajan's 2nd Volume of *The Geometry of Quantum Theory*, 1970, or Wightman's paper "Localizability of Quantum Mechanical Systems", *Rev. Mod. Phys.*, 34, 845-72. All of these give the precise theory, and are pretty mathematical. Then of course there is the treatment in Foldy and Wouthuysen, for a more hueristic account, in the case of spin 1/2.

ch3

I have quite a lot on this in my thesis; I hope to write it up for publication soon. I think the issue is fundamental. Some of it is in "Locality, imaginary numbers, the Bell inequality and relativistic quantum field theory"' which I've submitted to *Foundations of Physics*. I don't know yet whether they're going to

Levy Building Givat Ram
91904 Jerusalem, Israel
Tel 585652.טל
בנין לוי
גבעת רם ירושלים 91904

2151 Hartrey
Evanston, IL 60201
May 10, 1990

Dear Alex,

This time it is my turn to take so long to get back to you. No, I was not the referee for your complementarity paper. But I profited from rereading it, so thanks for the reprint.

I have a dim memory of having already expressed the following idea (hope) to you. But now, with my manuscript complete, perhaps there is a small amount further foundation for it. The thought is that by eliminating the individuatability of "particles" - let's call them quanta - we reduce the gap between field and "particle" concepts, which cicumstance, I am hoping, will go some way towards resolving the strain between the field and particle concepts which you describe so clearly in the context of GTR. Whether, or what kind, of "particles" one sees depends on ones state of motion. With the particle concept this seem just not to make any sense. If the blighters or individuatable, there being an absolute difference between "this one" and "that one", then they would have to be there or not, in an observer independent way. But once we let go of the individuatability, we can think of quanta as something we "measure", rather than count, even though the measure must be quantized. Then it is no longer so outrageous that what we see when we "measure" an "amount" of quanta should be detector dependent.

Here is another way of expressing what I think is the same thought. You comment (p., 575-6) that in the conventional understanding of QM there is some kind of observer dependence of properties, but not of whether or not there is a micro object or not. I claim that this gap between properties and "substance" lies in that aspect of our idea of substance which invovles what I call individuatabiliy (Transcendental individuality, primitive thisness, hacceity, etc). Let go of this interpretive element, (which is terribly obscure anyway!) and we can treat the question of what is present in the same kind of way in which we treat the properties of what is present.

Now it is my turn to unload on you. Here is a copy of the manuscript resulting from my book project. Much of this will be old hat ot you, but hopefully some of even the old hat you will find presented in in helpfully new ways. If you get a chance to look at it, be sure to send me any comments that come to mind.

At the end of the summer I will be moving to Davis where I will be joining the philosophy department. So put me down for a new mailing address as of September 1:

Department of Philosaophy
University of California at Davis
Davis, CA 95616

Do keep in touch, and in particular, send me things you are working on!

2407 Elendil Lane
Davis, CA 95616
November 13, 1990

Dear Bob,

I'm still trying to get myself settled here in Davis and so have not yet seriously gotten back to work on my book manuscript. But I have read your comments and thought about them a little. I will be thinking about them much more carefully when I finally get to doing the final draft. But here are my interim reactions.

Starting with your shorter more recent letter, I see what you mean about following the route you sumarrize to get a description of the quantum field in terms of states of the overall system. Why then all this fuss about "operator valued fields"? It's simply because many people out there (e.g., Redhead) seem to think of the "quamtum field" in this misleading and confusing way. So I set out to set that confusion straight, and doing that necessitated the discussion of determinables, field configurations, and the like. Apparently this is not a confusion you have ever been seriously tempted by. So you will not take much interest in this chapter.

Indeed, I felt uncomfortable about having said too little about why description of the interaction takes the form that it does. I apprecaite your wondeful summary. Can you give me references where I can read in greater detail about the intimate relation among gauge symmetry, charge conservation, and the form of the interaction, both so I can digest it more clearly, and to serve as a reference?

Turning to your earlier and longer letter, for this one I even more need to think about what you say in much more detail. But, again, here are some first reactions. Possibly you will have some simple things to say about some of them which might keep me from chasing up blind alleys.

You urge a different presentation of quantization, finding fault with the approach through an analysis into normal modes. First of all, don't forget that I also don't favor that approach! I want to start with the idea of non-individuatable quanta (chapters 1 and 2) and then use the quantum mechanics for indvidual quanta to get out the conventional formalism. I do this in sections 1-4 of chapter 3. Then, starting in section 5 I present conventional ways of quantization to show how one gets to the same thing which I came up with in section 1-4. I start with the approach through normal mode analysis, both because it is frequently used, and because, I have the impression, it was historically important. I do brielfy mention the approach you prefer. We disagree only in that I claimed that it really

applies only to quantization of real valued fields, while you claim that it is perfectly general. I claimed restricted applicability only on the grounds that the proceedure invovles thinking of the q as "coordinates", which would seem to require them to be real valued. Is your position simply this: why not generalize further and let these "coordinates" be complex valued? I don't see anything wrong with such a stance. But it would be nice if one could say something more. Alsok, Redhead claims that the two approach, where both applicable, are stricly equivalent. Do you agree? And if so, do you still feel strongly that one is preferable to the other?

Turning to the issue of how to treat the negative energy solutions on the approach you describe, k is interpreted, without argument or motivation, as the momentum energy for vector. This licences $k^2 = m^2$, which in turn licences the negative roots. At this point I'm not seeing the preferability of this, to citing the negative frequency solutions to the harmonic oscillator equation. The interpretation of k seems ad hoc. Perhaps there is more you can say about this. You also worry about how my line of thought will apply to the Dirac equation, since the Dirac equation is first order. But when we remember that the Dirac euqation comes about as a clever factorization of the second order equation, the apperance of a negative root is no longer mysterious (a point I didn't make in the manuscript, and probably should).

Here is another way to put my reaction. You complain that working in terms of normal modes obscures what is really going on - that I'm obscuring the fact that the origin of the negative frequency components is due to the invariant separation of the k's into the upper and lower light cones. But at this point I am finding the approach you prefer no less obscure - the "information" has been smuggled in by interpreting k as the e-m 4-vector, to be treated relativistically. Of course you then get two roots, but the question now becomes: why interprete k in that way? I'm not seeing the advantage.

At this point I don't understand the Cassemir effect nearly well enough to respond to the last part of your letter. I'm going to have to digest it much more thorougly and go back to do some more learning. The example you wrote out for me will be very helpful when I do that, and I can't tell you how much I appreciate your having taken the trouble.

What ever little I have accomplished in putting together my little book comes from a number of you working with me at great length helping me to understand this material more clearly. So, in every respect, I appreciate your efforts. If there is anyting quick and obvious to say in response to these preliminay thoughts, do let me know. Otherwise I'll get back to in a few months when I have thought about all of this much more carefully.

Cheers,

Paul

References from van F
On particle (non) identity

Stasi

---

de Muynck, W & van den Eynde (1984
Lapp, C (1986) "On the relation between
indistinguishability & identical particles
and (anti) symmetry of the wave function in Q
Synthese 67, 477-496

Peter Mittelstaedt Naming and identity in quantum logic
Proceedings of the 7th International Congress of
Logic Methodology and Philosophy of Science Salzburg

Dalla Chiara, M.L. (1983) and Toraldo di Francia
(1983) Individuals Kinds and Names in physics
Logica e Filosofia della Scienza, Oggi
V.2 49-72 ; also in Versus (40) (1985)
29-50

219 - 272 - 8603

1341 Squire Drive, Apt. H
South Bend, IN 46637
15 September 1990

Dear Paul,

Thank you very much for sending your manuscript -- quite a long time ago -- which I read with great interest. Catherine and I are spending the Fall semster (September to December) at Notre Dame, although only she has a job here. I had hoped to see you in Chicago sometime -- so I was a little disappointed when I learnt that you were going to leave Chicago for Davis. Anyway, I hope the change of departments does not interfere with your role as chairman of the PSA colloqium where I am supposed to give my paper. I am looking forward to meeting you in Minneapolis.

I still don't see how giving up on individuability of quanta can accomodate the observer-dependency of numbers of quanta, or, the observer-dependency of the vacuum state. This case is different from the case of states with unsharp numbers of quanta that you discuss and that your recipe seems to treat successfully. We know that the vacuum state in flat QFT is (and must be) unique; there are no alternatives. Obviously, removing individuatability from the concept of quanta does not help with situations where there are no quanta (for a specific observer).

You mention on p. 42f. that one of the reasons why the harmonic oscillator interpretation is not an ideal starting point is the fact that, in general, there are no harmonic oscillators in non-Minkowskian spacetimes. But if you really regard this as a serious problem you shouldn't try to go into the "reverse direction" and build up Fock space from one-quantum Hilbert spaces. This "alternative" suffers from exactly the same difficulty: in non-Minkowskian spacetimes there is, in general at least, no Fock space construction that all (geodescic) observers agree upon (cf. for instance, Saunders' article in Brown/Harré, p. 166).

Thus, if we really want "our theories... to have as great a generality as possible", we should probably abandon Fock space and embrace the algebraic approach to QFT. I hate to say this because I have such a poor understanding of this whole affair; and because it will make the task of the interpreter even more difficult. But I don't see how to avoid this conclusion. We should probably not base our interpretations on a formalism that proves more and more insufficient (insufficient in foundational research -- not, of course, in applications to experimental problems). As Saunders points out, correctly in my view, the real advances that are to be expected in tackling the measurement problem will probably come from the algebraists. And most of the recent work in QFT in curved space is done in

the algebraic fashion; cf., for instance, B.S. Kay, "QFT in Curved Spacetime." In: K. Bleuler et. al. (eds), Differential Geometrical Methods in Theoretical Physics. Boston 1988, 373-93. -- Haag's theorem which you mention as an interpretative problem for future work may be much more central to your enterprise than it seems at first sight.

I think you could easily insert a paragraph that addresses this problem and go on with the remark that your construction is valid for some chosen observer.

In reading the book I had the feeling that some more general remarks about the strategy of your argument, about what you are about to do in this chapter, for instance, would be helpful. This would be important in particular for readers who already have some knowledge of the usual textbook presentations of QFT; and I guess this group of readers will be your main audience. Thus, for instance, what you say in the conclusion to chapter 2 (p. 66) would have helped me a great deal if I had read it in the introductory paragraphs of the chapter. I understand that you tried to give some information about the relation of your approach to the traditional one on pp. 40-44. But somehow I couldn't really do much with this information before I had read the whole chapter. Therefore, an overview in the style of p. 66 would be helpful in these early pages.

I see that your way of building up the QFT formalism helps to keep classical fields and so-called quantum fields conceptually separate. (I like the discussion of fields on pp. 108ff. very much.) But there seems to me to be an important link between the concepts that you might emphasize more. What we are dealing with in the case of fields (classical or quantum) are systems with infinitely many degrees of freedom (this is, of course, implied when you talk about field theories attaching properties to spacetime points). This inflation of degrees of freedom has quite drastic consequences for the Hilbert spaces that we use to describe these systems; we get, for instance, super selection rules. I always found this emergence of exceptions to the superposition principle in QFT very impressive. Maybe it's only a matter of taste, but this feature of QFT is so different from what we know in Quantum Mechanics that I think an interpretive treatment of the theory should mention it.

p.111: Why can't we think of determinables -- just as of operators -- as mathematical entities? For instance, a determinable could be a function from objects into values of properties. What's wrong with this view?

p.138: The argument for normal ordering doesn't seem quite convincing. You say that "the need for normal ordering... arose in the context of a discussion introducing the interaction Hamiltonian..." Because, in this discussion, we didn't have the

finished theory -- we are about to set it up by "guessing" -- there is here no question of violating logic. But the need for normal ordering -- and that distinguishes it from the need for renormalization -- arises in the case of free fields as well; in fact, there it arose historically for the first time. In the free field case, however, your argument doesn't hold: Here we have the theory and we want to start calculating.

On the other hand, I don't really understand why you couldn't make the same argument in favor of renormalization. Why not say: Only after we introduce renormalized quantities in the Hamiltonian do we have the correct theory; before we do that we are still guessing to find the right Hamiltonian?

p.158: Bethe's original calculation of the Lamb shift was not only shockingly loose in the eyes of "some non-physicists" but in fact it seemed shocking to many physicists at the time: "Everybody was furious because Bethe had done it in an absolutely lowbrow dirty kind of way, and everyone knew, of course, that he had to be right." (This is Abraham Pais' recollection in L.M. Brown/L. Hoddeson (eds), The Birth of Particle Physics. Cambridge 1983, 276.)

In my copy of the typescript I have corrected the typos that I found. Since there are many of them, I could either send you my copy back or bring it to the PSA meeting if that kind of information is still useful to you. Let me know.

I am very much looking forward to meeting you in October.

Yours,

Alex.

oct 3, 1990

Dear Paul: You probably thought I'd forgotten your field theory manuscript since you sent it to me so long ago. But I didn't. I've read it several times and learned a lot from it - really, I enjoyed it a great deal. Besides forcing me to think hard about field theory, it gives me a model for my own book about string theory (if I ever get the chance to write it). Tim and I have discussed it and I know you will get his comments. Still, I thought I'd write you about what I think are the main problems - or anyway, what I think you ought to think about during supper. The main thing concerns two related notions - Field quantization and negative energies. I don't think you are right about what you call field quantization and I call canonical quantization. The method applies to all fields, real or complex or spinor, and is ~~exactly~~ the same method used to quantize classical particle mechanics. You identify the fundamental degrees of freedom q_i, their canonical momenta π_i and impose equal time commutation relations

$$[q_i, \pi_j] = i\delta_{ij}$$

In field theory $q_i \Rightarrow q(\vec{x},t)$, the field at a pt is a degree of freedom, and we read the lagrangian $\mathcal{L} = \mathcal{L}(q(x), \partial_\mu q(x))$ to find the ~~canonical~~ canonical momenta, $\pi = \frac{\partial \mathcal{L}}{\partial \dot{q}}$. I think focusing on the oscillators (normal modes) is misleading because ① they are not required for quantization - they do not appear in the quantization of a system of free particles, yet the method of quantizing free particles is the same as for fields - only the number of degrees of freedom is different, ② as you emphasize, the normal mode oscillators are not real (= physical) oscillators that exist in spacetime. By focusing on the oscillators you are led to call field quantization analogical. I disagree. It's no more analogical than quantizing a system of n classical particles. Further, I even think talking about oscillators is misleading since the momentum space quanta are plane wave states (free particles) and not hermite polynomials characteristic of a harmonic oscillator (That might be a question you could address in your book -

How come we get plane waves instead of hermite polynomials? Now I want to convert this point about field quantization with the negative energies + ideas how I look at the maths: Let's take a complex scalar field for ex (as you do): I write a plane wave expansion

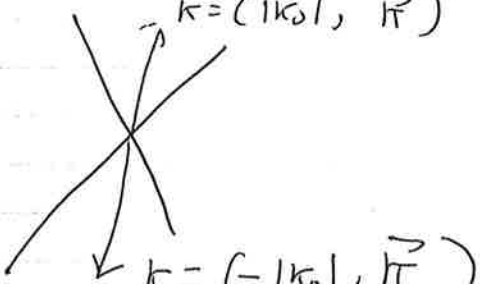
$$\phi(x) \sim \int d\kappa\, a(\kappa)\, e^{i\kappa x}$$

(I write $\sim$ instead of $=$ because I ignore factors like $\frac{1}{2\omega_\kappa}$ or $\frac{1}{(2\pi)^3}$)

Notice it's 4 dimensional, κ being a 4 vector. I want to emphasize from the beginning we are in relativistic spacetime. Now I am thinking of κ as being something which will get interpreted as an energy momentum 4 vector when we quantize (it's a wave vector now). So we know

$$\kappa^2 = m^2$$

and κ can either be in the upper or lower lobes of the light cone



the upper being the + freq ($\omega_\kappa + \omega_{-\kappa}$) κ's, the lower lobe contains the negative freq ($\omega_\kappa - \omega_{-\kappa}$) κ's. This is an invariant separation of the κ's so I can write

$$\phi(x) \sim \int d\kappa \left(a(|\kappa_0|, \vec{\kappa})\, e^{i\kappa x} + a(-|\kappa_0|, \vec{\kappa})\, e^{i\kappa x} \right)$$

since $\kappa^2 = \kappa_0^2 - \vec{\kappa}^2 = m^2$

$$|\kappa_0| = \pm\sqrt{\vec{\kappa}^2 + m^2} = \omega_{\vec{\kappa}}.$$

Also we have to make the appropriate choice of κ_0 in the $e^{i\kappa x}$ factors. Then we have

$$\phi(x) \sim \int d\vec{\kappa} \left(a(\omega_{\vec{\kappa}}, \vec{\kappa})\, e^{i(\omega_{\vec{\kappa}} t - \vec{\kappa} x)} + a(-\omega_{\vec{\kappa}}, \vec{\kappa})\, e^{i(-\omega_{\vec{\kappa}} t - \vec{\kappa} x)} \right)$$

Notice we now have a 3 dim integral over $\vec{\kappa}$, not a 4 dim

This because we have, in effect, integrated on κ_0 using the delta function

$$\delta(\kappa^2 - w_{\vec{\kappa}}^2)$$

Now since $w_{\vec{\kappa}}$ is always positive, I can let $\vec{\kappa} \to -\vec{\kappa}$ in the second term

$$\phi(x) \sim \int d\vec{\kappa} \left(\alpha(w_{\vec{\kappa}}, \vec{\kappa}) e^{i\kappa x} + \alpha(-w_{\vec{\kappa}}, -\vec{\kappa}) e^{i(-w_{\vec{\kappa}} t + \vec{\kappa} x)} \right)$$

$$\phi(x) \sim \int d\vec{\kappa} \left(\alpha(w_{\vec{\kappa}}, \vec{\kappa}) e^{i\kappa x} + \alpha(-w_{\vec{\kappa}}, -\vec{\kappa}) e^{-i\kappa x} \right)$$

So far, just classical field theory. Now we do some quantum mechanics (actually, first, from we can make the hermitian charge

$$\alpha(w_{\vec{\kappa}}, \vec{\kappa}) = b_{\vec{\kappa}} \quad , \alpha(-w_{\vec{\kappa}}, -\vec{\kappa}) = a_{\vec{\kappa}} \quad)$$

Then we have

$$\phi(x) \sim \int d\vec{\kappa} \left(b_{\vec{\kappa}} e^{i\kappa x} + a_{\vec{\kappa}} e^{-i\kappa x} \right)$$

and the ~~in~~hermitian field

$$\phi^*(x) \sim \int d\vec{\kappa} \left(b_{\vec{\kappa}}^* e^{-i\kappa x} + a_{\vec{\kappa}}^* e^{i\kappa x} \right)$$

On quantizing $\phi(x)$, I find

$$[a_{\vec{\kappa}}, a_{\vec{\kappa}'}^+] = \delta_{\kappa\kappa'}$$

$$[b_{\vec{\kappa}}^+, b_{\vec{\kappa}'}] = \delta_{\kappa\kappa'}$$

So $b_{\vec{\kappa}}^+$ is an ~~dest~~ annihilation op $d_{\vec{\kappa}}$, and $b_{\vec{\kappa}}$ is a creation operation $d_{\vec{\kappa}}^+$ and we have

$$\phi(x) \sim \int d\vec{\kappa} \left(d_{(\vec{\kappa})}^+ e^{i\kappa x} + a_{\vec{\kappa}} e^{-i\kappa x} \right)$$

$$\phi^+(x) \sim \int d\vec{\kappa} \left(d_{\vec{\kappa}} e^{-i\kappa x} + a_{(\vec{\kappa})}^+ e^{i\kappa x} \right)$$

So the negative freq and positive freq components become associated

with diffent quanta or particls. What are their relatin? The 2 diff partils have the same mass - that's determined by the lagrangn which they share, but if we compute the conserd charge of the they - there is a symmty $e^{i\alpha}$ which leads to a conserd ~~charge~~ current and so a conserved charge, we find that the 2 diff partils have the opposite charge - they are antipartils of each other. Now where do negative energs come into this? As far as field theory, nowhere. But if you are thinky of 1st quantized relativist quant mechanics you may wonder; how come there are no negative energy states in Field theory, when they do appear in relativistic quantum mechanics? The answer is, the $\phi(-\omega_\kappa, -\vec{\kappa})$ which would be a quanta of negative energy and neg momentum, is interpretd as a positive energy particle with pos momentum. But how can you can do that? Why does it work? The reason is, a negative energy, neg momentum particle spins s looks like an antipartile spinning s with positive energy and positive $\vec{\kappa}$. But why that? From the field theory pt of view, it just follows from the calculation of the conserved charges. But heuristically, we can see whats going on: Consider the conserd probabty current of the complex Klein Gordon eq of relativistic q.m.

$$j^\mu \sim e(\phi\partial_t\phi^* - \phi^*\partial_t\phi \;,\; \phi\vec{\nabla}\phi^* - \phi^*\vec{\nabla}\phi)$$

For $\phi \sim e^{i\kappa x}$

$$j^\mu \sim e(E, \vec{\kappa})$$

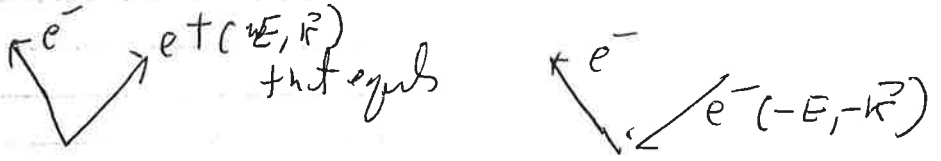
$$\phi \sim e^{-i\kappa x}$$

$$j^\mu \sim e(-E, -\vec{\kappa}) = -e(E, \kappa)$$

} neg by

so a negative energy, neg momtm particle of charge e looks like a pos E and pos κ particle of charge $-e$. This is why we have antipartils in field thy and no neg energs. But notice something very impt. This is also true in Relativistic quantum mechanics! As Feynman discovered,

Suppose I want to compute the amplitude for electron-positron ~~adjoining, coherent pairs~~ creation.



So I use the negative energy states, but with E & k have the opposite signs, to calculate antiparticle processes, i.e.,

$$\langle e^- | V | e^+(E,\vec{k}) \rangle = \langle e^- | V | e^-(-E,-\vec{k}) \rangle$$

The detailed justification for this comes from propagator theory, and works for both bosons & fermions. Thus, Dirac's picture is really not covariant-seeming heuristically - since it only makes sense for fermions and in ~~the~~ any case ignores the stuff about time reversal which comes from the propagator approach. My point is: the role of the negative ~~not~~ energy (neg freq) states is to become anti particles that's true in Field theory and relativistic quantum mechanics and harmonic oscillators have nothing to do with it in the sense that they are not essential to understand this - there are no oscillators associated with the Fock relativistic equations. From my point of view, explaining things in terms of oscillators obscures what's going on. Why? You get the negative freq (neg energy) from the K.G eq as follows (roughly) you start with

$$\phi(\vec{x},t) \sim \int d\vec{k}\; a(\vec{k},t)\, e^{i\vec{k}\cdot\vec{x}}$$

and apply the K.G eq to ϕ getting ~~...~~

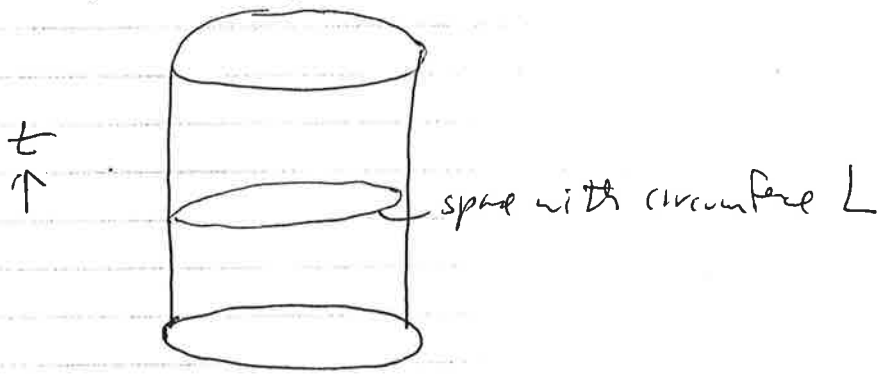
$$\ddot{a} + \omega_k^2 a = 0$$

which has two sol'n: $a(\vec{k},0)\, e^{i\omega t}$, $a(\vec{k},0)\, e^{-i\omega t}$.

You can then say that you don't get neg energies in nonrel q.m because the Sch eq is 1st order in time, ~~unlike~~ unlike the K.G eq. But then, what about the Dirac eq - its 1st order in time but has neg energies? You say, will in that case there are extra components. I think you have obscured the fact that the origin of the negative energy

components, is due to the same reason for all relativistic fields - the invariant separation of the k's into the upper and lower cone. (By the way, the K.G. equation does apply to each component of a Dirac spinor).

I think Tim covers most of the other points I would make as well - but let me elaborate one bit more - your comparison of the vacuum energy divergence with the loop integral divergences. Your point is that the former is not so serious since it's a matter of the initial formulation of the theory - we have to pick the $\hbar$ ordering of the operators, in the transition from classical to quant. Theory. But the latter concern the finished theory and can't just be thrown away. But I don't agree - you can't just throw the vacuum energy density away; This is shown by the Casimir effect. The difference between the two is that one arises due to normal ordering of the field Lorentz products in the free field, while the other is due to the fact that we have point particles in the interacting theory. However, in each case the divergence has to be regularized and renormalized and this can not be done in either one just by throwing away the infinity. Here is a sketch of a simple example: Consider two dimensional cylinder spacetime



In 4 dim minkowski spacetime the vacuum energy is $\frac{1}{2}\int d^3k\, \omega_{\vec{k}}$, for a real scalar field. Now suppose we have a massless scalar field in 2d cylinder, so $\omega_{\vec{k}} = |\vec{k}|$. Since ϕ must be periodic with period $\frac{1}{L}$, we know

$$k = \frac{2\pi n}{L}$$

for then $e^{\pm i(\omega_k t - kx)}$ will be periodic in L.

From $\langle 0|H_{int}|0\rangle = \frac{1}{2}\sum_k \omega_k$ (which is the above minkowski formula with only 1 degree of freedom)

massless field

and $\omega_n = |k| = \frac{2\pi |n|}{L}$

$$\langle 0 | H^{cyl}_{tot} | 0 \rangle = \frac{1}{2} \sum_{n=-\infty}^{\infty} \frac{2\pi |n|}{L} = \frac{2\pi}{L} \sum_{n=0}^{\infty} n \quad \text{which diverges.}$$

Now what we want to compute is

$$\langle 0 | \mathcal{H}_{cyl} | 0 \rangle - \langle 0 | \mathcal{H}_{min} | 0 \rangle$$

where $\mathcal{H}$ is the energy density. ~~Since we regulate~~ Now we introduce a regularization to make the sums finite. This will depend on a parameter α, so we will have

$$\langle 0 | \mathcal{H}_{cycl} | 0 \rangle (\alpha) - \langle 0 | \mathcal{H}_{min} | 0 \rangle (\alpha)$$

we normalize by letting $\alpha \to 0$, and we will get a finite expression which will be the difference between the vaccum energy density of the two spacetimes (at least due to a massless scalar field) Let $e^{-\alpha |k|}$ be the regulator. Then, since

$$\mathcal{H} = \frac{1}{L} H$$

$$\langle 0 | \mathcal{H}_{cyc} | 0 \rangle (\alpha) = \frac{2\pi}{L^2} \sum_{n=0}^{\infty} n e^{-\frac{\alpha 2\pi n}{L}}$$

This sum is easy to evaluate - it is of the form $\sum_{n=0}^{\infty} n x^n$, where

$x = e^{-\frac{\alpha 2\pi}{L}}$, and note that

$$x \frac{d}{dx} \sum x^n = \sum n x^n, \text{ and } \sum x^n = \frac{1}{1-x}, \; x < 1.$$

This gives

$$\langle 0 | H_{cyc} | 0 \rangle (\alpha) = \frac{2\pi}{L^2} \frac{e^{\frac{2\pi\alpha}{L}}}{\left(e^{2\pi\alpha/L} - 1\right)^2}.$$

Now expand the exponents in powers of α and using the binomial expansion. we get, after some arithmatic,

THE STATE UNIVERSITY OF NEW JERSEY
RUTGERS

Faculty of Arts and Sciences • Department of Philosophy
Davison Hall • Douglass Campus • New Brunswick • New Jersey 08903 • (201) 932-9861/9181

$$\langle 0|\mathcal{H}_{cyc}|0\rangle(\alpha) = \frac{1}{2\pi\alpha^2} - \frac{10\pi}{6L^2} + O(\alpha)$$

To get $\langle 0|\mathcal{H}_{cyc}|0\rangle(\alpha)$ we just let $L \to \infty$ in the expression for $\langle 0|\mathcal{H}_{cyc}|0\rangle(\alpha)$! Or, we are "assuming" that as $L \to \infty$, cylinder spacetime $\to$ 2d minkowski spacetime. Then

$$\langle 0|\mathcal{H}_{cyc}|0\rangle(\alpha) - \langle 0|\mathcal{H}_{mink}|0\rangle(\alpha) = \frac{1}{2\pi\alpha^2} - \frac{10\pi}{6L^2} - \frac{1}{2\pi\alpha^2} + O(\alpha)$$

$$= -\frac{10\pi}{6L^2} + O(\alpha)$$

and as $\alpha \to 0$ we have

$$= \frac{-10\pi}{6L^2}$$

~~Note that if $L \gg L'$,~~ ~~$\langle 0|\mathcal{H}$~~ If we now set our vacuum energy scale by letting $\langle 0|\mathcal{H}_{mink}|0\rangle = 0$, we have $\langle 0|\mathcal{H}_{cyc}|0\rangle = \frac{-10\pi}{6L^2}$ relative to that scale. Then note that for $L < L'$, $\langle 0|\mathcal{H}^{(L)}_{cyc}|0\rangle < \langle 0|\mathcal{H}^{(L')}_{cyc}|0\rangle$ and the influence of the vacuum energy density due to a massless scalar field is to make cylinder space contract. This is exactly like the Casimir effect instead of cylinder spacetime putting a boundary condition on the vacuum modes, one imagines conducting plates in the vacuum making the modes periodic so that $\int dk \to \sum_n \phi(k_n)$. The energy density difference between the vacuum with the conducting plates and without is a finite number and negative. Thus the plates will feel a force pushing them together. Anyway, my point is just that what's really going on is that you have a normal ordering constant, $\pm\int dk\, \omega_k$. It

THE STATE UNIVERSITY OF NEW JERSEY
RUTGERS

Faculty of Arts and Sciences • Department of Philosophy
Davison Hall • Douglass Campus • New Brunswick • New Jersey 08903 • (201) 932-9861/9181

has to be regularized and renormalized. That depends on the boundary conditions. In spirit it's the same as the ultraviolet renormalization. Another interesting example occurs in the free string. Here the infinite vacuum energy must be normalized to 1 for consistency of the theory. By the way, I'm surprised you did not discuss that while normal ordering of $\langle 0|N|0\rangle$ in minkowski leads to $\langle 0:N:|0\rangle = 0$, the vacuum fluctuations do not vanish, $\langle 0:\Delta\phi^2:|0\rangle \neq 0$. (note, there are free field fluctuations, not the virtual particle fluctuations in the interacting field vacuum).

Anyway, I hope these are useful remarks for you. Sometime, I hope I can write you about the renormalization paper - I've thought alot about it since I wrote you last and I think I'm a lot clearer (and understand what I want to say much better.) Again, I really enjoyed the manuscript,

Rob.

Oct 14, 1990

Dear Paul: I'd like to make a few more brief remarks - mostly about your chapter, "on what quantum fields are not". It's not that I disagree with your conclusion - but to me how you get there, via the discussion of determinables etc, is very confusing, and at this, leaves out some crucial questions. You worried about what's field like about the quantum field. But right at the start I would compare this with the question, for example, "what's oscillator like about the quantum harmonic oscillator - In the former case, what field is the that has a value at each space (or space-time) pt, in the latter, there don't seem to be any things with definite location oscillating back & forth. The answer in the latter case is two fold: 1st, the equations of the quantum harmonic oscillator are gotten by quantizing a classical harmonic oscillator - it's because of that, that it's called the quantum harmonic oscillator; 2nd, since we observe things in nature that appear to be classical oscillators, we have to be able to explain the appearance of these on the basis of quantum mechanics - Forgetting the measurement problem, that's usually done via Ehrenfest's Theorem: states of large quantum number (n) have expectation values which obey the classical equations. So when we see what looks like a classical oscillator, what we see is a large n state of the of the quantum oscillator. The case of the quantum field is similar: the equations of the quantum field are gotten by quantizing classical fields (or at any rate, the classical descriptions of). And like the harmonic oscillator, we explain the appearance of "classical fields" in the world in terms of certain states of the quantum field, whose expectation values behave like the classical fields. (your point and Bohr's as, I suspect I agree). But note, these states are not large n energy eigenstates, as in particle quantum mechanics. The electric field operator

$$\vec{E} \sim a + a^\dagger \quad \text{since} \quad \vec{E} \sim \frac{\partial \vec{A}}{\partial t} \sim a + a^\dagger$$

so for any state with a definite number of quanta

$$\langle n | a + a^\dagger | n \rangle = 0$$

So here's the 1st question I think you come out but should discuss: what states of the quantum field have expectation $\langle \alpha | \vec{E} | \alpha \rangle$ for a state that looks like a classical electric field? These are the coherent states, which are eigenstates of the annihilation operator $a(\vec{k})$. But there is subtlety The second question would then be - how come there are no classical fields in nature corresponding to or quantum fermi fields? The answer is that coherent states require superpositions of states

Why does operator valued fields don't build in? Fermions on path integral approach — also — I don't recall to discuss determinant...

Can't have many quanta, and such states are ruled out by the exclusion principle. So we can't build up suitable fermion coherent states. But if that is the correct explanation, we have the third question I think you should discuss. Namely, the only observed classical fields are the photon field (EM-magnetic field) and the gravitational field - and both are massless fields. And this is the subtlety I mentioned - why aren't these classical fields corresponding to missing boson quantum fields? Since as far as the exclusion principle goes we should be able to build up suitable coherent states.

This is how I would structure the chapter and the questions I would emphasize. But then, there is no necessity to discuss determinants etc. And also, I wouldn't spend much time talking about operator valued fields, for that is not an aspect of any of describing quantum fields. On the path integral approach, we study the integral over all classical field configurations (that satisfy the appropriate boundary conditions). Operator valued fields don't need to be mentioned. So one suspects they can't have ontological significance. (I should have said above that emphasizing the difference between boson and fermi fields is how we give a quantum explanation of the classical distinction between waves and particles. (or really, massless boson fields and the other fields). I think this is important to bring out.)

A final small pt from your interactions chapter. When you discuss the $e\bar{\psi}\gamma^\mu A_\mu \psi$ interaction term of the QED lagrangian, & you mention that invariance principles can help ~~to get it~~ are to arrive at it, but I think you underestimate the role of gauge invariance in providing an explanation for this interaction. Your discussion makes it seem like it came from thin air, almost, or from some analogies with no firm theoretical base. I don't agree. The right fact is, we know from observation that electric charge is conserved, and that the Dirac field (or Dirac equation) has a conserved current $J^\mu = \bar{\psi}\gamma^\mu\psi$, (can't find reference)

which is implied by the its ~~the~~ invariance under global transformations $e^{i\Lambda}$, Λ = constant. And so there is a conserved charge $Q = \int d^3x\, J^0$. Requiring this global symmetry ~~to be~~ become a local one, so that the action is invariant under $e^{i\Lambda(x)}$, then implies that the conserved current J^μ is coupled to the photon field A_μ, by $eJ^\mu A_\mu$. And we get an electromagnetic interaction that obeys charge conservation. Thus, there is an intimate connection between gauge symmetry, charge conservation and a certain form of interaction. In addition, this generalizes to a wide class of fields - not just the electromagnetic field. So I think we have a broad theoretical framework ~~for~~ ~~which~~

within which the ~~natural~~ Eqm interaction occurs. I think you give the impression there is no such framework within which we can understand Eqm type interactions.

Again, I've gotten a lot out of reading and thinking about your work

Rob.

DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

4.6.90

Dear Paul,

Thanks for your letter. Helene Simon is now back in Israel, before going direct to Harvard at the end of July. No additional point about the manuscript that occurs to me. I think you mention, though only in a footnote, that for an infinite set of harmonic oscillators, the commutation relations admit of non-equivalent representations, so in addition to the usual Fock space representation built by repeated application of creation operators to the vacuum state, there are "non-Fock" representations spanned,

DIRECT LINE: CAMBRIDGE (0223) 334556

UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

4.6.90

Dear Paul,

Thanks for your letter. I believe Simon is now back in Israel, before going direct to Harvard at the end of July. One additional point about the manuscript that occurs to me. I think you mention, & only in a postscript, that for an infinite set of harmonic oscillators, the commutation relations admit of non-equivalent representations, so in addition to the usual Fock space representation built by repeated application of creation operators to the vacuum state, there are "non-Fock" representations spanned,

an infinite number of particles.

There is a brief discussion of this in Schweber (pp. 163-164.) who gives some of the mathematical references. The proofs of non-Fock representations show, really, that the Fock approach is of broader generality than the particle approach, even in the non-relativistic case, where, on the basis of Fock-space representations only, they are held to be equivalent.

With best wishes

Rudolf

informally speaking, by states with an infinite number of particles.

There is a brief discussion of this in Schweber (pp. 163-164.) who gives some of the mathematical references. The possibility of non-Fock representations does, really, show that the Fock approach is of broader generality than the particle approach, even in the non-relativistic case, where, on the basis of Fock-space representations only, they are held to be equivalent.

With best wishes

Rudolf

DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

13.5.90

Dear Paul,

I was very pleased to hear your exciting news about the appointment at Davis. Richard Healey will be an excellent colleague, and there is so much going on, on the West Coast generally. I very much enjoyed my visit to San Diego, and hope to complete arrangements for an exchange arrangement with Cambridge.

After Southern California, Moscow was a very different sort of experience, but everything is changing so rapidly over there, and there seems to be an enormous interest in philosophy of science, and to learn more about Western ideas and approaches.

As I said on the telephone I have now had time to work through your

DIRECT LINE: CAMBRIDGE (0223) 334556

UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

13.5.90

Dear Paul,

I was very pleased to hear your exciting news about the appointment at Davis. Richard Healey will be an excellent colleague, and there is so much going on, on the West Coast generally. I very much enjoyed my visit to San Diego, and hope to complete arrangements for an exchange arrangement with Cambridge.

After Southern California, Moscow was a very different sort of experience, but everything is changing so rapidly over there, and there seems to be an enormous interest in philosophy of science and to learn more about Western ideas and approaches.

As I said on the telephone I have now had time to work through your

ATT

have made a very good job of the whole thing, but here are a few specific comments, together with a list of typos on a separate sheet.

p. 34 para 3, line 7 ff.

You say here that the oscillators do not correspond to any physical systems (besides oscillating in space-time. I think this needs more comment. The Fourier amplitudes $q(k)$ provide a set of "collective" coordinates for describing the configuration of the field. In this sense they are just as "real" as the 'displacement' amplitudes $q(x)$. Fourier amplitudes are just a special case of generalized coordinates for specifying the configuration of the field. You seem to suggest that they are somehow just mathematical fictions. But it is not unlike introducing the relative position and the location of the centre-of-mass for a 2-body system. Does it follow that there is nothing at the centre-of-mass

manuscript for the book. I think you have made a very good job of the whole thing, but here are a few specific comments, together with a list of typos on a separate sheet.

p. 34 para 3, line 7 ff.

You say here that the oscillators do not correspond to any physical systems themselves oscillating in space-time. I think this needs more comment. The Fourier amplitudes $q(k)$ provide a set of "collective" coordinates for describing the configuration of the field. In this sense they are just as "real" as the 'displacement' amplitudes $q(x)$. Fourier amplitudes are just a special case of generalized coordinates for specifying the configuration of the field. You seem to suggest that they are somehow just mathematical fictions. But it is not unlike introducing the relative position and the location of the centre-of-mass for a 2-body system. Does the fact

DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

II

make it an 'unphysical' conclusion?

p.38 I think a little more could be said here about the appearance of anticommutators and the related issue of the spin-statistics theorem. as stressed by Green (PR 90 (1953), 270) Anticommutators arise naturally as a possible quantization scheme, if one imposes the requirement that the Hamiltonian be the infinitesimal generator of time-displacement and remember that the Hamiltonian itself is a bilinear form in the creation and destruction operators. The spin-statistics theorem shows that if attention is restricted to commutation or anticommutation at space-like separation, which is sufficient to guarantee that bilinear forms like the Hamiltonian, etc commute at space-like separation, then no 'causal' field is constructible with the wrong association between spin and statistics.

p.T.O.

DIRECT LINE: CAMBRIDGE (0223) 334556

UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

11

make it an 'unphysical' coordinate?

p.38 I think a little more could be said here about the significance of anticommutors and the related issue of the spin-statistics theorem. As stressed by Green (PR 90 (1953), 270) anticommutors arise naturally as a possible quantization scheme, if one imposes the requirement that the Hamiltonian be the infinitesimal generator of time-displacement and remember that the Hamiltonian itself is a bilinear form in the creation and destruction operators. The spin-statistics theorem shows that if attention is restricted to commutation or anticommutation at space-like separation, which is sufficient to guarantee that bilinear forms like the Hamiltonian, at commute at space-like separation, then no 'causal' field is compatible with the usual association between spin and statistics.

P.T.O.

By a causal field one means one which commutes or anticommutes at space-like separation with its canonically conjugate field. The clearest statement of this point is in Weinberg (PR 133B (1964), 1318).

p42 Again you make the point that the Fourier analysis of the electromagnetic field is purely formal – but see my comments above. In fn 2 you refer to a different part of mine, that quanta of definite momenta are associated with travelling waves, rather than standing waves. For this point see also Fermi (RMP 4 (1932), 87).

In para 2 you bring in the spin-statistics theorem again, but without any attempt to explain what is going on (see above).

p. 89 You say here that field quantization assumes that the classical field is real valued. But a complex field can always be regarded as a pair of real fields.

p. 90 line 2 – Perhaps you could add

one which commutes [illegible] and commutes [illegible] space-like separation with its canonically conjugate field. The clearest statement of this point is in Weinberg (PR 133B (1964), 1318).

p42 Again you make the point that the Fourier analysis of the electromagnetic field is purely formal – but see my comments above. In fn 2 you refer to a different point of mine, that quanta of definite momenta are associated with travelling waves, rather than standing waves. For this point see also Fermi (RMP 4 (1932), 87).

In para 2 you bring in the spin-statistics theorem again, but without any attempt to explain what is going on (see above).

p. 89 You say here that field quantization assumes that the classical fields are real valued. But a complex field can always be regarded as a pair of real fields.

p. 90 line 2 – perhaps you could add [illegible] "known to do with gauge invariance and

DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

III

the associated peculiarities of photon spin"

p. 97 - 98

Similar point as above re reality of oscillations in field quantization

p. 98 as above re quantizing a complex field

p 108 End of para 2 as above re quantizing a complex field

p. 126 Hoag's theorem only denies the existence of the interaction representation if it is still possible to work in the Heisenberg representation. The result is essentially mathematical, concerned with the non-existence of certain 'density operators'. It is no more 'serious' than the physicist's use of the S-function!

P. T.

DIRECT LINE: CAMBRIDGE (0223) 334556

UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF HISTORY AND PHILOSOPHY OF SCIENCE

FREE SCHOOL LANE
CAMBRIDGE CB2 3RH
TEL: CAMBRIDGE (0223) 334540

FROM HEAD OF DEPARTMENT

PROFESSOR M. L. G. REDHEAD

III

the associated peculiarities of photon spin"

p. 97 - 98

Similar point as above. on reality of oscillators in field quantization.

p. 98 as above re quantizing a complex field

p 108 End of para 2 as above on quantizing a complex field

p. 126 Hoag's theorem only denies the existence of the interaction representation. It is still possible to work in the Heisenberg representation. He must be essentially mathematical, concerned with the non-existence of certain 'density operators'. It is no more 'serious' than the physicist's use of the S-function!

p.T.O.

p. 148 para 2, line 2.
perhaps 'quanta' should read 'particle'?

p. 152 1st 2 lines
See above for comments on Fourier analysis. It all depends what you mean by "parts"!

I am convinced you should go ahead and publish under your own name. The work of drafting has been entirely your own, and you are characteristically generous in your reference to myself and Gordon in the introduction. The important thing is that this material should be available to philosophers, not specifically who writes it.

With very best wishes

Richard

" perhaps 'quanta theory' ...

p. 152 1st 2 lines
See above for comments on Fourier analysis. It all depends what you mean by "parts"!

I am convinced you should go ahead and publish under your own name. The work of drafting has been entirely your own, and you are characteristically generous in your reference to myself and Sorbon in the introduction. The important thing is that this material should be available to philosophers, not specifically who writes it!

With very best wishes

Richard

---

p.145	Last line walking	should read... Walking
p.146	line 5 diagnomatic	diagnomatic
p.147	line 12 desentlen	desention
p.152	line 8. Co-ordinate longitudinale bail nonoxide	Coordenate monoxide
p.153	line 6 putting	putting
p.154	line 2, para 3 invdividual	individual
p.155	line 6 superimpied	superimposed
p.156	line 6 and represtent	are represent
p.157	3 lines below figure electomagentic	electromagnetic
p.158	line 4 Bocher last line para1 slight line 1, para 2 muscests	Bothe sleight physcens

p.145	last line	
	walking	shoved road walking
p.146	line 5	
	diagnomatic	diagnomatic
p.147	line 12	
	description	description
p.152	line 8	
	co-ordinate	coordinate
	longitudinal line	
	nonstide	monside
p.153	line 6	
	putting	putting
p.154	line 2, para 3	
	invidividual	individual
p.155	line 6	
	superimposed	superimposed
p.156	line 6	
	and	as
	represtent	represent
p.157	3 lines below figure	
	electomagentic	electromagnetic
p.158	line 4	
	Bother	Bothe
	last line para 1	
	slight	sleight
	line 1, para 2	
	physists	physicists
	line 3, para 2	
	precedents	precedents

Princeton University Press
41 WILLIAM STREET
PRINCETON, NEW JERSEY 08540
TEL.: (609) 258-4900
TELEX: 510-685-2306
FAX: (609) 258-6305

August 13, 1990

Professor Paul Teller
2151 Hartrey
Evanston, IL 60201

Dear Professor Teller,

Thank you for sending in a copy of your vita, which I have now added to your file. Ann has already sent your manuscript to one reader, and I am in the process of tracking down a second one. We will be in touch with you again as soon as we hear something from our reader.

Ann is on vacation until August 20, but if you have any questions before she returns, please do not hesitate to call me at (609) 258-4930.

With best wishes,

Yours sincerely,

Trishka Waterbury

Trishka Waterbury
Assistant to Ann Wald

Philosophy and
Political Theory Editor

Notes on boundary conditions
Century Transformation of in, out fields

$a(k,t)$ $a^\dagger(k,t)$

as $t \to -\infty$ time dep because simple $a(k)e^{-i\omega(k)t}$

~~interaction is dropping~~ interaction drops out as $t \to \pm\infty$

when $a(k,t)$

$$\lim_{t\to-\infty} a(k,t)\, e^{i\omega(k)t} = a_{in}(k)$$

because when only field bearing free in limit.

$$\lim_{t\to+\infty} a(k,t)\, e^{i\omega(k)t} = a_{out}(k)$$

State with 3 a-quanta at finite t: $a^\dagger(k_1)$

finite time

$$a^\dagger(k_1,t)\, a^\dagger(k_2,t)\, a^\dagger(k_3,t)\,|0\rangle$$

using for $|t| \gg 0$, because want any-any like state

in in in $|0\rangle$

Where are effects of interaction in the in/out fields?

$$\psi(x',t') = \psi(x,t) + \int_{-\infty}^{+\infty} d^3x''\, dt''\, dx''\, P(x', x, x'', t', t'') \times j(x'',t'')$$

$$\lim_{\substack{t\to\infty \\ t'\to+\infty}}$$

$\to \psi_{out} = \psi_{in}$ + effects of interaction

$S_{\alpha\beta} = \langle \beta_{out} | \alpha_{in} \rangle$

1312-973

-2

$a^*_{in}(k)\ a^*_{out}(k')\ a^*_{out}(k'')\ |0\rangle$

1312-973-2132

How does one reconcile the H= of molecular interpolation of $a(k)/a^*(k)$ as k/n operators for pure momentum with interpretation of a_{in} and a_{out} as falling apart?

$a^*_{in}(k)$ adds k momentum to an out state, e.g. $a^*_{(out)}(k')\ a^*_{out}(k'')|0\rangle$, but in a complicated way, not just by adding a k-momentum state.

Try: ψ_{in}/a^*_{in} are still H operators, giving scattering in a time dependent way, but leaving out the delta function ϵ.

Try: looking at how ψ_{in} changes over time in terms of advanced & retarded green fns.

Try: reinterpreting the passive transformation as an active transformation

Research questions

1) What is the connection between the path integral formulation of QFT the harmonic osc. approach & the book's approach. What does the path integral approach seem to say physically. Note that one when we start with the path int approach we get to Feynman diagrams, & so we want to say that they "say the same thing".

2) Gaderi's point about alternative bases. What, if anything, is left out in the Harmonic osc. intro? Do we get additional? repetative? consistent? complementary? physical description when we discuss in alternative bases?

General Form for Quantum Theories

1) Theories of Quantum Systems
2) Each system can be in various states
3) There are static eigenstates of observables
4) States include superpositions of (3)

Application to Quantum theory of quanta.

1) No states include quanta
2) Include states with indefinite quanta #

2 problems.

a) How interpret the zero point fluctuations & zero point energy
b) How interpret the fact that in 10_{cm}^3 there is a bath of Rayleigh quanta - through non-definite ones! -
c) How the Rayleigh debacle is a Planckovski radiator.

I The Minkowski vacuum $|0_M\rangle$

A Not an eigenstate of of $\hat{\phi} = \sum F_i \hat{a}_i + \bar{F}_i \hat{a}_i^\dagger$

B. $\approx$ Ground state of an HO not an eigenstate of $\hat{x}$

II Rindler quantization

A. Represent ϕ in another basis

B, Transform to proper time of a uniformly accelerating observer.

1. $\phi = \sum_i \left(f_i \hat{b}_i + \bar{f}_i \hat{b}_i^\dagger \right)$

C. Equating these ($\hat{\phi} = \hat{\phi}$) we get

1) $a_i =$ (22.10) p. 297

2) F_i positive frequency with respect to the Minkowski time translation parameter, t,

3. But F has a negative freq component w.r.t to the time parameter τ,

4. Then β^I_{ij} and $\beta^{II}_{ij} \neq 0$

5. $|0_M\rangle \neq |0_R\rangle$.

6. $|0_M\rangle = \prod_j \{ A_j \sum_{n_j} B_j |n_j, I\rangle \otimes |n_j, II\rangle \}$

Not correlation between region I and II

7. So $|0_M\rangle$ a Rindler detector sees a "thermal bath"

III Detection / Radiation

A. A Rindler particle detector is seen by an inertial observer as a radiation of Minkowski particles.

B. For $|X_{in}\rangle = |\downarrow;\psi\rangle$

$|X_{out}\rangle = |\downarrow;\psi\rangle - i\vec{b}_1(0)|\uparrow;\psi\rangle$, $\vec{b}_1(\gamma)$ is the region I Rindler annihilation operator for a particle of Rindler mode γ

C. $\hat{b}_1(\gamma) = \hat{a}(\Gamma_1) + \hat{a}^\dagger(\Gamma_2)$, for some M-modes Γ_1, Γ_2

D. So for $|in\rangle = |\downarrow;0_M\rangle$,

$|out\rangle = |\downarrow;0_M\rangle - i\|\Gamma_2\||\uparrow,1\rangle$, i.e. the detector acts like a Minkowski radiator with the transition from $|\downarrow\rangle$ to $|\uparrow\rangle$!

$\|\Gamma\rangle$ is spread out over all space.

E. ~ an EPR phenomenon.

1. Excitation of detector in I correlated with region II effects
2. Correlations exist in the analysis of $|0_M\rangle$ in the Rindler basis (see 22.12, p. 297) (3)

12) Feynman talks about particles taking all paths from start to finish. Somewhere, Feynman comments that thinking this way resolved for him the question of how the particles knows "which way to go" Tim takes this as evidence that with at least one hat on, Feynman thinks of this realistically . He can't remember the reference, but thought it wan's either QED or Laws

13) Parts: I see two formal difference between mereological parts and analytic parts (vector components) To get an analogy going one needs to analogize disjoint parts with components along orthogonal axes. Otherwise one gets into trouble with saying things like going both north and south are parts of standing still. But then, for a vector in a 2-d space, there can only be two non overlapping parts. and with merological parts one can get into parts of parts, but this goes wrong for vectors.

A second difference is that a mereological part has its own s-t trajectory, not shared with any non-overlapping part. Not so for analytic parts. Tim comments: But quantum systems don't have exact trajectories. I comment: But they do have s-t tubes of "fuzzy position"

Ch 5 or 6

Say why one "would expect an electron to interact with its own field - (superposition) probably ~~that all she~~ all discussion of classical self interaction should go in ch 6.

Ch 5

Page 123

Why should one expect an electron to interact with it's own field?

How reconcile normal ordering & $\frac{1}{2}$ dimension of vac expectations with appeal to vacuum fluctuations used in explaining the Casimir effect, etc?

We normal order the interacting hamil bcause density, which generates time evolution $\mathcal{H}(x)$ so that $\langle :\mathcal{H}(x): \rangle_0 = 0$. But in explaining C. effect we look at products $:\mathcal{H}(x): :\mathcal{H}(y):$ (check) We don't normal order the product. If $\mathcal{H}(x)$ has $\hat{a}, \hat{a}^*$, standing alone, the product will have $\hat{a}\hat{a}^*$, i.e won't be normal ordered.

11) Is the peverse reading really pervers? Tim comments that for tangent vectors in a tangent space there is only comparison given a connection, and then only along a curve. So we really do assign different quantities to each of the s-t points. More generally, whenever there is a gauge freedom, since the gauage can be set locally, we again have a different quantity at each point. Remember that we can set the gauge locally and compensate with the gauge field. He agrees that for scalar fields the "peverse" reading is peverse. But what all this shows is that this issue already comes up in classical theory — it's not new with the issue of the quantum field. This at least needs , first of all, to be acknowledged. Then, it compromises the arguemnt as stated. Since we already know from the classical case that some fields have this perhaps unexpected aspect, it can't be straightforwardly a reason for not taking operators assigned to s-t points to be values of a quantity.

10) Tim found my "two kinds of space" unclear. He recommends instead talking about what it is for a particle to have a position. This may be more complicated than trivial assignment of a value of the s-t parameter.

Ch 3

Problem - Redhead 2nd ground with Redhead's Ozone, p 11 old PSA paper.

Redhead explains 2nd ground: start with n-particle Sch - it gets harmonic osc. Energy spectrum - so like a collection of HOs - but "just" another way of describing the n-particle theory. But also more general: constraint N=n is lifted.

Compare with Schweber?

Problem: How is interaction described. For the n-particle Sch theory, the subspaces of which particles how which momentum is moved around over time by interaction - the corps become time dependent. But how is this reflected in the HO transcription or Redhead first description?

Perhaps compare with Heisenberg.

FROM : XAVIER ROQUÉ, SEMINARI D'HISTÒRIA DE LES CIÈNCIES

Bellaterra, 5 març 1991

Benvolgut Jordi

M'alegro d'haver rebut notícies teves. La veritat és que no sabia que tinguessis intenció d'anar a Londres, ni tampoc que hagueressis estat treballant amb el Galison. Veig que treballes molt i que has entrat de ple al vertigen doctoral dels Estats Units. Aquí les coses segueixen un curs més tranquil, a cops massa tranquil, però bé la seva gràcia. — Pel que fa a dins, només he trobat la referència següent, a Weiner (ed.) Proceedings of the International School of Physics "Enrico Fermi" (Varena 1972): History of Twentieth Century Physics, Academic Press 1977. p. 129:

Potser "commuting" és una versió d'una comunicació personal que m'ha esmentat el Dorfel, però d'aixó no hi ha evidència textual.

Well, the situation which I had got into now was that I was dealing with these new variables, the quantum variables, and they seemed to me to be some very mysterious physical quantities, and I invented a new word to describe them. I called them *q*-numbers, and the ordinary variables of mathematics I called *c*-numbers, to distinguish them. The letter *q* stands for quantum, or maybe queer (QUEER) and the letter *c* stands for classical or maybe commuting. Then I proceeded to build up a theory of these *q*-numbers; *c*-numbers can be looked on just as a special case of *q*-numbers, having the property that they commute with everything.

Estic preparant un viatge a Estats Units per setembre - octubre d'aquest any. En principi visitaria el Center for History of Physics de l'Am. Inst. Phys. a Nova York i la Office de Berkeley, amb la idea de recollir material d'interès per la meva tesi. Ja te'n parlaré en més detall per carta: segueixo preferint-les al FAX per qüestions que no són urgents.

Una abraçada i fins una altra

XAVI

PD 1: Em vaig quedar sense saber què estaves editant perquè la darrera línia es va perdre.

PD 2: Vas telefonar el dia 12/2/91 però el vaig rebre després 4/3; i va sortir de Davis el 1/3/91 a les 11:28.

Ch II

I Harmonic Oscillator:

A. $F(x) = -\frac{\partial V(x)}{\partial x} = -x$

B. $V(x) = \frac{1}{2} x^2$; $K(x) = \frac{1}{2} p^2$

C. $H = \frac{1}{2}(p^2 + Q^2)$;

D Quantize: $[\hat{Q}, \hat{P}] = i$

E. $\hat{a}, \hat{a}^*$

1) $\hat{a} = \frac{1}{\sqrt{2}}(\hat{Q} + i\hat{P})$

2) $\hat{a}^* = \frac{1}{\sqrt{2}}(\hat{Q} - i\hat{P})$

3) $\hat{Q} = \frac{1}{\sqrt{2}}(\hat{a}^* + \hat{a})$

4) $\hat{P} = \frac{1}{\sqrt{2}}(a^* - a)$

F $[\hat{a}, \hat{a}^*] = 1$, from $[\hat{Q}, \hat{P}] = i$

G $H = \frac{1}{2}(aa^* + a^* a) = a^* a + \frac{1}{2}$.

II $\hat{N} = \hat{a}^* \hat{a}$

A Eigenvectors $\hat{N}|n\rangle = n|n\rangle$.

B $\hat{a}^*|n\rangle$

C $\hat{a}|n\rangle$

D $n = \langle n|\hat{N}|n\rangle = \langle n|\hat{a}^* \hat{a}|n\rangle \geq 0$

E. So n, integer ≥ 0. :

1) $a|n\rangle = |0\rangle$;

2) $a|0\rangle = 0$.

III Fermions

A $[\hat{a}, \hat{a}^*] = 1$

B $\hat{N} a^*|n\rangle = (1-n)\hat{a}^*|n\rangle$

$N a|n\rangle = (1-n) a|n\rangle$

IV Start with Fock Space – Bosons.

A. $\hat{N}_k = n_k |n_1 \cdots n_k \cdots\rangle$

B Let $\hat{N}_k = \hat{a}_k^* \hat{a}_k$ with

1) $\hat{a}_k |n_1 \cdots n_k \cdots\rangle = C(n_k) |n_1 \cdots n_k - 1 \cdots\rangle$

2) $\hat{a}_k^* |n_1 \cdots n_k \cdots\rangle = C^+(n_k) |n_1 \cdots n_k + 1 \cdots\rangle$

3) ~~$C_k^- C_k^+ = n_k$~~ $C^+(n_k - 1)\ C(n_k) = 1$

4) $(a_k a_k^* - \hat{a}_k^* \hat{a}_k) |\cdots n_k \cdots\rangle$

$= n_k + 1 - n_k = 1$

Ch 2.

Some felt that at the end of Ch II it wasn't clear what point I was trying to make.

7) Reference on discussions of heicceities:
Armstrong, Combinatorial theory of possibility. A strong and week notion of hecceity, only of of which bear logically on PII. - See chapter 4

Lewis, in Plurality of worlds - a section against heccities.

8) In arguing the relation of hecities and superposition: in individuating two particles, I say that the number-labels create puzzles about superposition. Tim comments that the labels can't be simply dropped cold - in say, treatment of the singlet state in fock space, one will have to replace the number lables with individuating properties, like right and left.

9) More on heccities: Hecceities do many jobs which need to be separated and sorted out. Tim comments that his notion of heccities is one concerning that which can be freely combined with properties. He takes this to be a stonger notion than mine, which has to do with labeling. In addition Tim identifies three different ways in which one can think of heceities:

1) A principle having to do with reassigning properties to "it"
2) A principle having to do with reidentification in possible worlds.
3) A principle having to do with counting.

Tim also comments that meaauring (in my sense) won't support diachronic individuation. When we have TWO trajectories, with reidentification of parts of the trajectories, we have something stronger than "two" without such reidentification.

Ch 1

~~Phy as a source for Metaphysics. — comment on the~~

Comment on Physics as a source for metaphysics/ontology.

Questions for Seminar
Ch 1

1) Reboul confused by my saying that LTI is a "thing" in virtue of which labels attach

2) Really time identity over time is a 3rd notion covered by subsalience - need to untangle. This for LTI & PTI.

3) What do labels do, really a modal notion like the switch notion. Do I have a linguistic notion - labelability, and a metaphysical/modal notion - switchiness - if so - it seems sounds like two notions, not two ways of getting at one notion

4) LTI & Supervenience - what basis is LTI to underwrite supervenience? (cleaner for indefinite pronouns #), but not at all clear when pronoun # is fixed. (E.g. underwriting cont'l) Symmetry not further hampered by ± TI; we can suppose there are 2, baptized particles with indiscernible properties in the orthodox QM sense.

5) Does mereological empiricism conjunctivism force any superfluous structure to be strongly superfluous?

6) 3 ways of introducing LTI: 1) Support for labels 2) Premature thesis re same entity after properties are stripped 3) switching properties → a new
a) Isn't 2 & 3 really the same?
b) 1) looks like a non-modal condition; (2) like a modal condition, so don't these introduce different ideas?

For chapter 1.

Start with "conceptual distinguishability": Objects are conceptually distinguished by a theory if the theory uses referring labels (1, 2, 3) ~~abbreviate~~ predicates properties $P_\alpha(1)$, $P_\alpha(2)$ ... and counts permutations as distinct states.

The question then arises: What is the relation of conceptual & observational distinguishability. Need to characterize obs dist. In classical mechanics they coincide - well do they? For I want to talk about switching the labels/particles under the trajectories. We can say: Let particle one be the one which in fact has ... trajectories - in this sense they coincide.

In QM they clearly don't coincide, even in this sense

Chapter 1.

1) Substitute for the word undecidable?

2) Prior metaphysical prejudice?
   a) How strong is it
   b) classical intuitions?

3) p 8÷9 Characterization of TI

4) p 9. if no TI, PII trivially true (?)

5) "Same" vs "Distinct Hilbert spaces.

6) p 28–29 Reader's call for comparing with literature on theory choice & underdetermination of theory by data - e.g. comparison with Friedman
   a) do absolute velocity
      i) Strongly superfluous
      iii) Weakly superfluous
      iv) Neither?

7) p 31 p?, 116-8 How do the labels + symmetry, also work to keep track of how many particles have which property without a concept of which particle has which property

8) p 33 last line: & implicitly reject Trop theory.

9) Tim's comment to p 35. So far commutators are put in by hand., what's the advantage of arbitrarily imposing commutators vs arbitrarily dropping and fixing LTPHSB

10) p 37 "We can't make sense of the idea of particles with LTI superimposing to give the nucleon (anti) symmetric state."

But we have superposition anyway. What sense is meant here? What is the additional problem. Particularly, what does LTI have to do with it?

11) Alex R's question: Differently accelerated observers see different fields. No unique vacuum that all observers agree on. I hope that reasoning industrially from the fields would help with Unruh - but not clear how, especially in the case of ~~the~~ no fields

Can labels be used as an auxiliary device, without LTI, to describe (anti) Symmetric states?

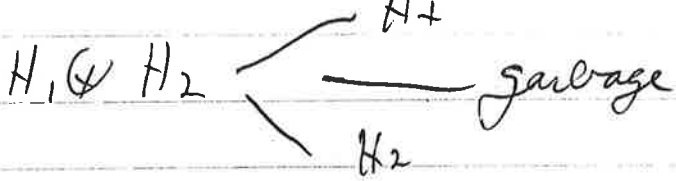
A principle of compositionality, that the meaning (interpretation) of the whole is determined by the meaning (interpretation) of the parts would seem to block such an account. If $|a(1)\rangle|b(2)\rangle + |b(1)\rangle|a(2)\rangle$ has an interp which corresponds to that of the summands we would have to have an interp for $|a(1)\rangle|b(1)\rangle$ and $|b(1)\rangle|a(2)\rangle$. For these not to be neutral, these would have to be different, & would seem to be block to LTI.

Could $|a(1)\rangle|b(2)\rangle + |b(1)\rangle|a(2)\rangle$ be understood holistically? The numerals "count" up the # of quants, give the distribution of properties. But very unclear how this is to be understood, especially under the requirement that the summands do not come to the same thing.

Are the commutation relations, put in by hand, a blemish = trait of Surplus structure?

Choice of commutation relations is motivated only by the requirement of numerical adequacy [modulo spin statistics thm]. But the resulting theoretical structure is itself not methodologically problematic.

Or the LTPHSF has the same arbitrariness + surplus structure, for we have



Here too we must make a choice of the relevant subspace on grounds only of empirical adequacy — also by hand. Here we also have the surplus structure!

Relation to strong & weak unverifiability.

Richard points out that strong unverifiability is relative to an encompassing theory. In Newtonian mechanics the assertion that the sun is at absolute rest is unverifiable strongly. But not when Maxwell equations are added, for Galilean transformations spoil them.

I say that $\exists \psi = |a(1)\rangle |b(2)\rangle$ is unverifiable since if true we would see no Bell violations. This involves an assumption on the a.p. distribution. As van Fraassen shows, one can accommodate such states by tinkering with the a.p. distribution.

Doesn't this make it weakly unverifiable?

I have to check how this works as described by van F. Does he really? I know he can accommodate LTI, but does he do this by excluding non-symmetric states

Ch II

I Harmonic Oscillator

A. $F(x) = -\frac{\partial V(x)}{\partial x} = -x$

B. $V(x) = \frac{1}{2} x^2$; $K(x) = \frac{1}{2} p^2$

C. $H = \frac{1}{2}(p^2 + q^2)$;

D Quantize: $[\hat{q}, \hat{p}] = i$

E. $\hat{a}, \hat{a}^*$

1) $\hat{a} = \frac{1}{\sqrt{2}}(\hat{q} + i\hat{p})$

2) $\hat{a}^* = \frac{1}{\sqrt{2}}(\hat{q} - i\hat{p})$

3) $\hat{q} = \frac{1}{\sqrt{2}}(\hat{a}^* + \hat{a})$

4) $\hat{p} = \frac{1}{\sqrt{2}}(a^* - a)$

F $[\hat{a}, \hat{a}^*] = 1$, from $[\hat{q}, \hat{p}] = i$

G $H = \frac{1}{2}(aa^* + a^* a) = a^* a + \frac{1}{2}$.

II $\hat{N} = \hat{a}^* \hat{a}$

A Eigenvectors $\hat{N}|n\rangle = n|n\rangle$.

B $\hat{a}^*|n\rangle$

C $\hat{a}|n\rangle$

D $n = \langle n|\hat{N}|n\rangle = \langle n|\hat{a}^* \hat{a}|\rangle \geq 0$

E. For n, integer ≥ 0. :

1) $|a|1\rangle = |0\rangle$ '

2) $a|0\rangle = 0$.

III Fermions

A $[\hat{a}, \hat{a}^*] = 1$

B $\hat{N} a^*|n\rangle = (1-n)\hat{a}^*|n\rangle$

$N a|n\rangle = (1-n) a|n\rangle$

IV Start with Fock Space – Bosons.

A. $\hat{N}_k = n_k |n_1 \cdots n_k \cdots\rangle$.

B Let $\hat{N}_k = \hat{a}_k^* \hat{a}_k$ with

1) $\hat{a}_k |n_1 \cdots n_k \cdots\rangle = C(n_k) |n_1 \cdots n_k - 1 \cdots\rangle$

2) $\hat{a}_k^* |n_1 \cdots n_k \cdots\rangle = C^+(n_k) |n_1 \cdots n_k + 1 \cdots\rangle$

3) ~~$C_k^- C_k^+ = n_k$~~ $C^+(n_k - 1)\ C(n_k) = 1$

4) $(\hat{a}_k \hat{a}_k^* - \hat{a}_k^* \hat{a}_k) |\cdots n_k \cdots\rangle$

$= n_k + 1 - n_k = 1$

individuality. This fits quite well with the analogy of waves offered on p. 33. Waves don't *always* have diachronic individuality: when they interact, questions of identity with the incoming waves is moot. But waves *sometimes* have such individuality (when there is no interaction). Similarly, a single quantum particle, like a electron, seems to be individuatable when not interacting with others (think of an electron in a Penfield trap). It is notable that the question of statistics only arises in states of equilibrium, i.e. in states that are the product of extensive interaction. If identical quantum particles do not maintain individuality through time when interacting, the appropriateness of Fermi or Bose statistics may be vindicated.

✓ It is worthy of some consideration that David Bohm's interpretation of quantum theory is capable of recovering Fermi and Bose statistics even though Bohm postulates classical particles, particles which have definite positions at all times and follow definite trajectories. Thus the classical notion of a particle can be reconciled entirely with all characteristically quantum mechanical phenomena. In the case of statistics, this is done by denying the equiprobability of distinct states, and the equiprobability fail to hod because of the non-local quantum potential. A Bohmian QFT can similarly predict all quantum effects while postulating the existence of a perfectly definite field configuration at all times.

✓ In any case, the attempt to foist LTI on classical metaphysics is both ill supported and unneeded. The important point is that LTI is implied by the tensor product formalism.

✓ p. 9, l. 11 "be". First paragraph: LTI is not *needed* for labeling of particle to make sense as the classical case shows. All I need to do is attach labels at a time and the diachronic continuity will carry them forward. Second paragraph: the denial of the PII does *not* commit one to LTI. The relevant attributes for the PII are taken to be purely qualitative. I may hold that individuals are individuated by particular relations *to other individuals*, and hence by non-qualitative properties. I can maintain that Black's two spheres are two but deny that exchange of them *through all time* would result in a distinct situation. Further, the fact that an entity's individuality is not *fixed* by its qualitative attributes does not imply that it is *independent* of those attributes, as the gloss suggests.

This depends on how PII is interpreted, I will clarify further.

✓ p. 10- Note the "admit of reidentification" of para. 2- this is just diachronic individuality.

✓ p. 11- Again, I deny that classical physics must admit the possibility mentioned in para. 2. We don't need more than "ersatz LTI".

✓ p.12, l.1- "bare"; last para.- again, "particle" does not carry connotations of LTI; l. 2b-"properties represented by".

✓ p. 14, l. 3b- "represented"

✓ p. 15, bottom to top of p. 16: please provide an example of the "different statistical predictions" mentioned. Further, more support must be given to the claim that LTPHSF describes too many states: this really requires the close discussion of surplus structure.

p.16, l.2- "supposed"; l. 3b "exhaustive"; bottom of page- some justification for the equiprobability assumption is needed. This is, in general, a difficult issue, and is just glided over here. Clearly this whole discussion depends on it.

p.16. While the reviewer is right, this is a whole new research topic I will say a little more, but neither I nor anyone else can do this universal assumption justice in our current state of knowledge.

✓ p. 20, l. 3- "exemplified"; l. 6b- "roundabout"

✓ p. 21, l.3 -"compromises"; l. 8 - "restriction"

✓ p. 22- the idea that if two particles have identical *intrinsic* properties then all observable must be symmetric under label exchange is problematic. Suppose I have an electron in a box here and another in a box on the moon. Then the observable according to which the one here has property A and the one on the moon has property B is distinguishable from the one where the particle here has B and the other has A. This is so even though they are intrinsically identical. This of course breaks down if the two particles interact. Perhaps you mean to argue that I cannot know whether the one I put into the box here is the same one that comes out of the box here- but at least *prima facie* this seems to be something I can know in some cases.

In physics position is regarded as an "intrinsic" property, I will clarify

p. 24, l. 11- "other words"
p. 27, l. 6b- "contains"
p. 28, l. 8- "symmetric"; bottom of page- the example of the black hole does not seem to be one of surplus structure. If there are no black holes, or if black holes are not physical possibilities, then the GTR is just false (breaks down at some point). For the conditions that lead to the formation of black holes according to the theory are surely physically possible. So if nothing physically possible corresponds to the mathematical singularities, the theory is just wrong.

I think the reviewer is wrong on this. But I'm not a GTR expert - I'll check on this.

p. 30, l. 11- "labels"
p. 31, l.7b- "structure"; the mention of Fock space here deserves at least a footnote. The reader who has taken up the book to *learn* QFT will be shocked to find that a previous familiarity with the theory is presupposed. One should be reassured that the formalism will be explained in the next chapter.
p. 34, p. 8- "labels"
p. 35, l. 12- "characteristics"; second para.- the appeal to the commutation relations here deserves some comment. The Hilbert space proponent may say: "My theory is fine. It is part of the theory that one does not have non-symmetric states as representatives of reality." To this your response seems to be that one must justify such exclusions. One wants to see what the formalism says about the ontology, and bare exclusions need interpretation. But if we ask how the Fock space formalism manages to ensure the right symmetry properties, the answer seems to be: through the imposition of commutators or anti-commutators. Similarly, the statistics are derived from this. So it seems appropriate to ask what ontological significance the commutation relations have. Where do they come from? What do they tell us about the nature of the items in the theory? Otherwise, they look like another arbitrary restriction on the theory, one put in by hand to get the right numbers. If so, the advantage of Fock space over the simple exclusion of non-symmetric state in LTPHSF is obscure.

The reader is right that there is a lacuna in the argument & I'm particularly grateful for this opportunity to tighten and strengthen my argument.

p. 37 "we can't make sense of the idea of particles with LTI superimposing to give the needed (anti)symmetric states". I don't see what kind of "sense" is required here, and what LTI has to do with it. Superposition is a characteristic quantum phenomenon that appears even when only one particle is at issue. Think of the two-slit experiment. In some way, this is hard to make sense of, but LTI is no part of the problem. In general, the claim is repeated that superposition is somehow more comprehensible if we give up LTI but the connection is never really made.

Same reaction as my last one.

p. 38- given the comments above on the importance of commutation relations in this formalism and the lack of any ontological interpretation of them, the admissions on this page are welcome. But the promise that "We will return to this issue on a future occasion" is not fulfilled in this book (I think). Some discussion would be welcome.

This is a whole new research problem. It will be clearly described as such.

Chapter 2

p. 42, l. 4b- "exploiting". General comment on the harmonic oscillator analysis: I don't think that the quantization of the electromagnetic field via a normal mode analysis is given a sufficiently clear or sympathetic presentation. One can give a general recipe for quantizing a classical theory: analyze the classical system as having a certain number of independent degrees of freedom; write down the conjugate momenta corresponding to those degrees of freedom; associate operators with these canonical positions and momenta; impose commutation relations on the operators. From this point of view, the normal mode analysis of the electro-magnetic field is quite natural. What are the degrees of freedom of a classical field? One might be tempted to take the field value at each point as an independent degree of freedom, but this is incorrect if there are continuity constraints on the field. All continuous field configurations get a unique Fourier analysis, and the normal modes *are* independent degrees of freedom. Note that when using box normalization, switching from the field-values-at-a-point to the normal modes

While the description sketched here is correct, the reader does not address the limitations I describe in the text (which other readers have been at pains to point out to me!)

goes from an non-denumerable set of variables to a denumerable set. This indicates how continuity imposes constraints on the field values. Thus the harmonic-oscillator analysis in terms of normal modes is an almost inescapable consequence of regarding the object to be analyzes as a *continuous* field.In any case, it may be more convenient than using the field values at each point as the degrees of freedom of the system. This of course does not suggest reifying the normal modes into vibrations of some entity. I will have further comments on this in reference to chapter 5.

I'll explain.

p. 43, top- what exactly is the "privileged role" of the momentum basis? Why wouldn't the conjugate momenta associated with any set of independent degrees of freedom work as well? This should be explained. Also, "privileged" is misspelled.

p. 44, l. 7- "lowering"

p. 26, l. 11 et passim- "one-quantum" rather than "one quantum"?; l. 12 "dispersed"

p. 48, l. 7-"additive"

p. 49. l. 12b, 10b, 7b- "additive"; l. 4b-close parenthesis is needed.

p. 53, l. 7-"Bosons"

p. 57, l. 1- Should some remarks concerning the uniqueness of the vacuum be given?; l.11- the parenthesis here is never closed.

p. 62, l.6- "mathematics"; l. 1b- "particle"

p. 63, l. 11b-"eigenvalue"

Though it may not be apparent from the reader's wording, he agrees with me that the "recieved view" is problematic. But he says- that I correctly missed the too have know he's bonf. I about much ht but rid this, of time to need think it through. Those comments will lead to an important improvement.

p. 64, bottom para.- The claim that the claim that in QM position is an operator while in QFT it is a parameter is misleading is misleading (sorry). That is, there seems to me to be a very good reason to make exactly this distinction, a reason which is not brought out in this discussion. This issue is rather important, and I will likely return to it later.

In QM the position variable is really a degree of freedom of the system. That is, position is always *position of a particle* (in labeled Hilbert space) or *position of some quantum or other* in Fock space. This distinction is easily missed if one focuses on the one-particle QM, but is clear in two-particle QM: there there are six position operators, three for each particle. This is reflected in the familiar fact that the wave-function is a function on configuration space, not on physical space. In the one-particle case, the configuration space of the system is isomorphic to physical space, so the distinction can be easily overlooked. This whole discussion does not seem to make this important distinction.

The importance of configuration space in QM also raises a different means of approach to some of the questions addressed in chapter 1. If a system contains identical particles, especially particles which fail to have diachronic individuality, what exactly is the configuration space of the system? I think the point about labeled Hilbert space might be usefully illuminated in this way. If the configuration space of a system with identical particles is smaller than that of a similar system without identical particles (due to the indistinguishability of some of the states)\then use of the larger configuration space may be the source of surplus structure. Since the wave function lives in configuration space, this is a very natural question to ask. It may make contact with the interpretive issues of chapter 1 in a more direct way than discussing LTI.

In QM one can ask questions like- Where is particle A (suppose A to be the only particle of a given kind)? The fact that this can get different answers reflects the fact that the position of particle A is a degree of freedom of the system. Since particle A may not have a definite position, we associate an operator with this degree of freedom. On the other hand, in QFT on does not ask any experimental questions of the form Where is x? Rather, one asks questions like "What is the value of the field *at* space-time location x? The location is thus *not* a degree of freedom, the field value at a location is. Here the locations *are* in physical space. There are no operators for the location of anything, only operators associated with locations, operators for field values or particle numbers, etc. Thus space-time location is a parameter in field theory, not an operator.

There may be some sense in which saying that in QM position is an operator and not a parameter in misleading, but the point made above about the degrees of freedom of the

system, and about the role of configuration space in QM, are essential. It is, I think, a serious mistake not to clearly lay out the truth that lies behind the supposedly suspect dictum. More anon.
p. 66, l. 14- "presentation"; 5b- "attractive" & "ontology"; 1b "call".

Chapter 3

p. 68 first Para.- Are the disclaimers about technical considerations to be ignored needed?
p. 69- I hope that the sense in which QFT associates an operator with each space-time point is clear from my above discussion. The degrees of freedom of a field involve quantities at a given space-time point which can vary: associated with each of these is an operator. This is of course not to say that the *operators* are "out there" in the world. Rather it is to say that a quantum field has a value at a point in space-time in just the same sense as a quantum particle has a position. In each case, the classical theory always assigns a definite value while the quantum theory associates an operator which gives only expectation values for a given state. l. 5b- "generally"
p. 70, l. 2b "where"
p. 71, l. 2b "independent"
p. 72, l. 10- "detail"; l. 11 "should"; l. 12- "U*|**k**,0>; footnote l. 2- "operator"
p. 73, l. 6b- "a"
p. 76, l. 4- "relativistic"; l. 10b- "Alternative"
p. 77, l. 3- "description"; l. 4- "involving"; l. 5 "description"; l. 8- "oscillators"; l. 9- "description"; l. 10 "describing" l. 10b- "interpret"; l. 7 ff.- I don't see that the argument for obscurity has been made. I think the gloss given above is pretty clear.
p. 78 top para.- there has been a notion creeping in that there is some problem about *superposition* in the traditional approach to QFT or many-particle QM which is resolved by the acceptance of quanta as non-individuatable, an issue quite different from that of surplus structure. I don't see the connection. Superposition is an archetypal quantum effect, one for which there is no classical analog. The problem of interpreting superposition arises quite independently of any problem about individuating particles: think of the two-slit experiment when only one particle is involved. There is no formal problem with superposing states in the same Hilbert space in LTPHSF. There is a problem in superposing states of different particle numbers, but there is a more obvious problem of evolving into states with different particle numbers. If there is more than a formal problem, i.e. an interpretational question about the meaning of superposition, then I don't see how giving up individuatability helps us solving it. If there is only a formal problem of having states with different particle numbers in the same Hilbert space, again I don't see how the renunciation of individuatability is the key to solving this. l. 4- "freed"; l. 7- "alternative"
second para.- It is pleasing rhetorically to see the elimination of individuatability from the classical conception of particle as giving us a mid-ground between wave and particle. But 1) as argued above, if the property in question is LTI, classical particles don't need it. 2) if the property in question is diachronic individuality, why isn't what is left just a classical wave, which, as you yourself state, does not always have diachronic identity? What is *left* of the classical particle? l. 5b- "approach"
p. 79, l. 6b- "quantizes"; l. 4b- "numerically"; last line- quantum waves are not complex valued functions of space and time, they are complex valued functions on configuration space. As noted above, this confusion is hard to avoid in the one-particle case, but the distinction is essential. Also in last line-"complex".
p. 81, middle para.- I can't follow the reasoning about why the heuristic can't be given a rigorous reinterpretation in QFT. Bottom of page following- see note above on the "myth". l. 11- "equations"; l. 14 "substitution".
p. 83, l. 2b- "Schrodinger"

Discussed in Ch 4.

I'll make forward reference to Ch IV

I'll address this & I'll both clarify the situation & strengthen the argument. I don't know if I'll completely satisfy this reader.

I don't understand the comment; I don't say that the functions are funs. of a single s-t variable.

The gap between the reader and myself is smaller than he realizes. Though in order, the Dirac = is a factorization. I will clarify to make clearer what is happening here.

p. 84, l. 4- "seems"; The tenor of the discussion of positive and negative energy solutions that follows here uses a heuristic: negative energy solutions are not avoidable if the basic field equation (such as the Klein-Gordon equation) is quadratic in time. Although the heuristic is tempting , it is limited and perhaps does not give the best explanation of the origin of the terms. It is easy to see that this must be so since the negative energies also appear from the first-order Dirac equation. Further, if the solutions were mere artifacts (as in the "Annie is half as old as the square of her age 10 years ago" problem) then they could just be discarded as such. It is more natural to begin with a relativistic space-time and an analysis of the field into plane waves with time-like momentum four-vectors. one then gets the usual analysis into positive and negative frequency/energy equations by a change of the integration parameter on the negative frequency part. It is important to approach the question this way so that one can see the necessity of the negative frequency solutions: without them we don't have a complete basis for decomposing the field.

p. 85, l. 7b- "positive"; footnote l. 1- "frequency"; l. 6- "treatment". The interpretation of id/dt as the energy operator needs some justification. The Hamiltonian is the generator of infinitesimal time translations, but is not always the energy operator of the system.

p. 86, l. 11b- "Gordon", "inner"

p. 87, l. 1b- "matrices"

p. 90, l. 10b- "Hamiltonian"

p. 91, l. 2- "satisfying"; l. 5b- You don't want Psi to be an operator, do you?, l. 2b- "reinterpreted"

p. 92, l. 15- "frequency"

p. 93, l. 15b-"particle"; 12b- "frequency"; 6b- "momentum"; 2b- "tantamount"

p. 94, l. 1- "positive"; l. 8- "corresponding"; l. 3b- "Ultimately"

I'm in a bind here since other technitions, whom I respect (Redhead, Fleming) have insisted on these qualifications. But it will add clarity to answer the questions this reader poses here.

p. 96, l. 3- "presentations"; l. 5- "quantization"; l. 7- "solutions"; l. 6b- "valued". Comments on this last paragraph and following- this is what I was waiting for all along. Two questions. First- why is this only valid for a real-valued field? Second- why use "position" in scare quotes as opposed to, e.g., "degree of freedom"? In general, as is obvious, I don't think that the power of this approach to understanding QFT has been given adequate attention. This gives us a formally exact analogy between the transition from classical mechanics to quantum mechanics and the move from classical field theory to QFT. There may be puzzles about the sense in which a quantized field is really a field, but they are exact analogs to the question of the sense in which a quantized particle is a particle. The comment on bottom of p. 97 about how this line of approach is "at best, analogical only" is obscure to me. Of course there is no question of material oscillators being subject to quantization. But the purely classical Fourier analysis of a field into normal modes is also not a matter of resolving it into material oscillators. The resolution into independent degrees of freedom is exact, and the quantization of these is by the usual means.

p. 98 l. 2- "parasitic"; l. 6- "thoroughgoing". In this para. I don't know what the cash value of "more nearly physical" is.

p. 99, l. 2- "quantized"; l. 8- "absence"

p. 100, l. 7- "noteworthy"; l. 14- "positive"

p. 101, l. 10b-"positive"

I've been critically brief here - I'll clarify.

p. 103, l. 3b- "superposition". The argument here is a bit obscure to me. If the $|x\rangle$ are not localized in a Lorentz invariant way and the $|x\rangle_{NW}$ are, why not think that, if reality is Lorentz invariant (as it were) then the only true representation of localized states is via the $|x\rangle_{NW}$? I don't see how "two sorts of physical space" get into the analysis- rather there is only *one* sort of physical localization. It will, of course, be differently expressed in different bases. Of course, we might just abandon the notion that

localization is an objective (frame-independent) property of a state- this is not too implausible in any case.
p. 104, l. 7- "reference"

Chapter 4

p. 106- Clearly, the analog of a specific field configuration in classical field theory is not the set of field *operators* of quantum field theory, but rather the *state* upon which the operators operate. Similarly, the analog of the state of a classical particle in quantum mechanics is not the position *operators* but the state upon which the operators operate. In classical mechanics, each state of the system specifies the positions of the particles, in QM only probabilities are determined by the state. Similarly, in classical field theory, the state specifies the exact values of the (spatio-temporally indexed) field variables; in QFT the probabilities for these quantities are returned by allowing the associated field operators to act on the state. Again, QFT stands to classical field theory as QM stands to classical mechanics. The field *operators* are not the analogs of the field, they are the analogs of the classical *functions* that map the state of the field to definite quantities for particular field variables. Hence I deny that the operators represent a physical quantity in QFT- the quantities are represented by the state.
p.107, l. 7- "different"; the second paragraph- the idea that non-self-adjoint operators can represent physical quantities (better- physical degrees of freedom) via the real and imaginary parts separately is obscure to me. How could these be measured?
p. 108, l. 11b- "some"
p. 109 ff. I would replace "determinable" with "degree of freedom". In classical theory, each degree of freedom takes a definite value in all physically possible states; in quantum theory this is not so. The formal representation of a degree of freedom in quantum theory is an operator, the representation of the specific state, the analog of a classical field configuration, is the ket. Thus, again, the main difference is that in quantum states the degrees of freedom are not all assigned definite values. l. 3b- "configurations"
p. 111, second para.- there is a funny shift here from ontology to mathematics. Classical field theory has mathematical objects that represent physical states. There is then a mathematical function from states to real numbers that specifies the value of a particular quantity in a given state. Similarly, QFT has such a function- not to real numbers but to probabilities over measurable sets of real numbers. Thus there are clear *mathematical* analogies. The question of what in reality corresponds to the mathematical entities is quite important, but comparing ontology with mathematics in the two cases is not a fair comparison.
p. 112, l. 11- "ignore"
p. 113, l. 11- "picture"
p. 115- I hope the above presents a reasonably clear alternative to this picture. l. 11b- "temporally"
p. 116- I don't at all see what is "perverse" about this interpretation of the classical field. If I understand correctly, the perversity arises because one regards the field not as the instantiation in various places of one universal property but as the instantiation of physical quantities that are tied to each space-time point. But this is just what classical theory (including GTR) says! Think, for example, of tangent vectors. In the Euclidean context, one may mistakenly think that the same tangent vectors may be instantiated at different points. In the non-Euclidean manifold, however, we see that in general the notion of comparing vectors in the tangent spaces of different points is not well defined. Vector-types are confined, as it were, to a tangent space; we can't see vectors at different points as instances of a single universal. Comparison of vectors is only possible with the addition of a connection, and even then the vectors cannot be compared *per se* but only as transported along a path. Much more about the nature of gauge theories could be said here. l. 5- delete "a"; l. 2b- "breaks"

I believe the reader is wrong about this but clearly my argument needs to be more clearly stated, to get the point across to this perceptive reader!

This is a side issue, nothing on the book turns on it, so I want to leave it open.

I prefer to retain Term degrees of freedom but explain the connection with the idea of a degree of freedom. I took myself to be making exactly the reader's point here. I'll try to reword more clearly.

Yes, but I respond according to the above

But comparisons (identity) are for possible E&M field, which is the example in question. Still the point about GTA is well taken. Such a move needn't be "perverse". I'll take this good observation into account.

p. 118, l. 9- "determinable"; l. 11b- "<Phi|"
p. 119, l. 15- "(1),"

Chapter 5

p. 126, l. 7- "after all"
p. 128, bottom- To call the perturbation series expansion "wishful thinking" seems quite unfair. It is, of course, only useful for small perturbations, but the general method is quite respectable in those cases. I feel that some more tangible results are needed to justify the rather dismissive tone here.
p. 129, l. 2- "constructive"; footnote l. 3- "strength"
p. 137, ff. The discussion of "discarding" the vacuum field expectation values suggests that this is just a stop on the way to formulating the correct theory and so the vacuum expectation corresponds to nothing real. However, some physical phenomena, most notably the Casimir effect, appeal to the vacuum expectation. It is not clear that zero vacuum expectation is physically correct.
p. 139, l. 5b- "infinite"
p. 140, l. 5- "Feynman"; l. 6- "mileage"
p. 145, l. 1b- "walking"
p. 146, l. 5- "diagrammatic"; l. 7- "diagrammatic"; l. 11"representing"
p. 147, l. 12b- "description"; I don't really understand the objection to using the terms "creation" and "annihilation". Even if one gives up LTI, the result of the operator acting on a state is to yield a state with one more or less quantum, thus creating of annihilating one. Nor, so far as I can see, does the fact that states can superpose effect this. Thus I find the discussion on the next page unconvincing.
p, 149- One may not want to take the sort of ontology that Feynman associates with his diagrams seriously, but at least the position can be given a more sympathetic rendering. Feynman sometimes speaks as if the diagrams really do represent actually evolving processes, processes that all take place simultaneously and the sum of which yield the probability amplitudes for observations. These underlying processes would involve creation and annihilation of particles, exchange of mesons, etc. The smooth and deterministic evolution of the resultant amplitudes is not because there are no such creation and annihilation events at the underlying level but because the discontinuities are washed out when we sum up all the contributions. l. 4b- "do"
p. 150, l. 12- "amplitude"; l. 14-"constitutes"; l. 2b- "non-relativistic"
p. 151 ff.- This discussion of parts is, to me, unconvincing. It seems to me that the Fourier components of a wave are as legitimate parts of it as parts of a table are parts. This can only be refuting by providing an analysis of parthood, which is obviously lacking. The analogy to burning sugar on p. 152 is out of place: the Fourier components literally sum to the wave while the combustion products do not make up sugar. It may not be sufficient that the putative parts sum to the object for parthood, but it is pretty obviously necessary.

The analogy on p. 153 is also unfair. If I ask for an analysis of a wave into Fourier components the result is uniquely determined by the wave, it I ask for a resolution of a velocity into the sum of a Northerly and Southerly velocity the result is not fixed. In general we may distinguish two sorts of parts of an object: 1) analytical parts- the result of some unique method of regarding the whole as a sum of parts where the decomposition is into orthogonal components. Analysis of vectors into orthogonal components satisfies this, as does Fourier analysis. Analysis into non-orthogonal components does not. Thus, if we wish to analyze a motion into motion North plus something else we should begin by taking the projection of the velocity vector on a North pointing unit vector. Your actual velocity cannot, therefore, have as parts Northerly and Southerly components. 2) dynamical parts, such as are discussed, where the parts are identified via their causes. I don't see any objection to analytical parts as parts- neither

the sugar nor velocity examples tell against them. Nor is it relevant that a wave can be analyzed by resolution into many different sets of waves. I have a top half and a bottom half, a right half and a left half. I can be cut up in different ways, but these all yield parts of me. This is where a general discussion of mereology is most needed.

p. 153, l. 6- "putting"

p. 154, middle para.- "No hidden variables" proofs are irrelevant here- the question is in what sense the processes depicted by the Feynman diagrams are thought of as existing. According to Feynman they *all* take place. Thus Feynman would not refrain from saying that the particle passes through slit A or slit B- indeed he would insist that it does *both*. Its observed behavior is a result of the joint effect of all the happenings. l. 7b- "individual"

p. 155- considerations of how gigantic the superposition of these possible histories in the sum-over-histories picture is would not move Feynman. The more the merrier. Indeed, it is exactly because in some sense *every* possible history is tried that the view is so appealing: no selection principle among histories is needed. The particle doesn't have to know where to go, it goes everywhere possible all at once. Again, you may not like the picture, but at least it should be presented in all its glory. Thus the "as if this were not enough" rhetoric would not faze Feynman. l. 6- "superimposed"; l. 9b- "Feynman".

p. 156, l. 6- "an", "represented; l. 7- "density", l. 11- "corresponding"

p. 157, l. 4b- "catastrophe"

p. 158, l. 4- "Bethe"; l. 7- "non-relativistic"; l. 8- "Bethe"; l. 12- "Feynman"; l. 8b- "psychologists"

p. 159, l. 11- "suggesting"; l. 8b- "lattices"

Again, I wish the reader had been given me a reference to Feynman. I don't read him like this

Again, these reactions will guide me in reworking my argument.

Chapter 6

The only comment I have on this chapter regards the justification of the requirement of renormalizability given on pp. 247 (journal pagination) and 255. It is said that a scheme is hopeless if it requires an infinite number of empirically determinable constants. This seems not always to be so. For example, it was once speculated that the families of quarks would constitute an infinitely ascending hierarchy. There is no reason to assume the masses of the quarks would be calculated from first principles. But this is no fatal objection to the theory. If each successive family has a smaller effect on observable phenomena, approximations may be calculated to any desired degree of accuracy. No doubt an infinity of quantities needed to absorb the divergent terms would not be so benign, but one should explain exactly why this is so. Certainly one cannot reject the infinity *per se* since the real infinities approach is accepted as a possible interpretation of the physics.

This point will be easy to clarify; - the # of parameters would be completely unconstrained.